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THE COMPLICATION EXPERIMENT AND RELATED PHENOMENA.[1]

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I. Experiment with Myself as Subject.

While engaged as a subject in Dr. Burrow's experiments on complications and purely visual combinations[2] I was impressed with one fact which seemed to me significant. The moving pointer or index seemed fairly definite at the moment when I succeeded in obtaining a judgment which was subjectively satisfactory; the pointer was not absolutely sharp and distinct, it is true, but it was much more definite than I would expect, at the speeds of rotation employed, if the eye were really stationary.

This observation suggested that my fixation was not maintained at the moment of the apparent alignment, *i. e.*, of the coincidence of the pointer with the radius of the fixed mark, but followed the pointer for a very small distance. This possibility was suggested by consideration of the well-known phenomenon which appears when the eyes are moved across a disc of black and white sectors revolving fast enough to blur; frequently one of the sectors will flash out strongly, seeming to stand still momentarily. This *flashing sector* phenomenon is explained by Dodge[3] as due to a brief coincidence of the rate of movement of the sector with the rate of movement of the eye, the time of coincidence being long enough to give a sharp retinal image of the sector. Evidently, if the fixation, in Burrow's experiment, should deviate in the direction of movement

[1] From the Johns Hopkins Psychological Laboratory.

[2] Burrow, N. T., 'The Temporal Position of a Momentary Impression, etc.,' 1909, Psychol. Rev., Psychol. Monographs, Vol. XI., No. 4.

[3] Dodge, R., 'The Participation of the Eye Movements in the Visual Perception of Motion,' 1904, Psychol. Rev., XI., 1–14.

of the pointer even for an exceedingly short distance, a passable image of the pointer might be obtained.

The observation I have just described did not thrust itself upon me until towards the close of Burrow's work, for which reason largely, it led to no experimental issue in his hands, although communicated to him. I therefore decided to investigate the phenomenon myself, and did so in the period included between the middle of May and the middle of July, 1909. No subjects were available during that portion of the year so that I was obliged to perform the experiments on myself, as described below.

I used Burrow's apparatus[1] (with the modifications I shall describe), it being eminently suited to the work. My first endeavor was to find some means for steadying the fixation, and after casting about among several devices I finally adopted a piece of ordinary white mosquito-netting stretched over the half-dial of the apparatus and the half of the disc adjacent to the dial, as close to the surfaces of disc and dial as could be without rubbing on the disc. By this means I was provided with excellent fixation objects where the threads of the netting crossed the black fixed mark or 'goal.' The netting had meshes about 2.5 mm. square.

My first observation, on attempting to make judgments with the apparatus at one of the standard rates used by Burrow (1.25 sec. per rotation) was that *the meshes of the netting blurred* as the pointer went by the goal, although there was no other sign of disturbance of fixation. Evidently, there was either movement of the eye, or change of accommodation. My first efforts were directed to the maintainence of the sharp image of the threads, and after several hours' practice — on several days — I attained this fairly well; that is, I could keep the meshes reasonably distinct in most of the tests. But *the pointer now blurred* as it passed the fixed mark. This preliminary practice, and the succeeding work, were very trying to my eyes: so much so that very little experimentation, sometimes only a half-hour's work, could be done on one day, and my eyes were in an irritated condition for several weeks after the experiment concluded.

[1] Burrow, *op. cit.*, pp. 23-27.

In place of the discrete stimuli (flash of Geissler tube or stroke of sound-hammer) employed by Burrow, I substituted the click of a telephone receiver held in a clamp at about six inches from my right ear, as I stood in a position to make judgments. The telephone receiver was actuated by the 110 v.d.c. lighting current, with one carbon filament lamp in series, and a low resistance rheostat (about 50 ohms) in parallel with it. Contact was made through the mercury cup on the time machine, which was so adjusted that the travelling contact-point barely brushed the very crest of the convex mercury surface. In this way there was no appreciable interval between make and break of contact, hence practically a single loud click in the receiver; and there was almost no spark at the mercury cup.

My first set of results consisted of 100 determinations; for each of the rates .75, 1.25 and 1.75 secs. per rotation. Ten 'A' and 10 'B' determinations at one rate constituted the work for one day. My method of experimentation was as follows: Having moved to one side the lever which controlled the relative position of the discrete stimulus (*L* in Burrow's diagram and description), and having the contact-arm over the mercury cup, and the pointer approximately opposite the lever, I seated myself and shifted the pointer an unnoted distance towards the fixed mark, and then started the motor. As, after I had shifted the position of the pointer, I did not see the disc until it had gained its normal speed of rotation, I did not know the *starting position* of the click, *i. e.*, the position of the pointer at which the click occurred as I began to make judgments in any given test, nor did I know the position of the finger-lever corresponding to actual simultaneity of click and alignment.

After completing the determination, *i. e.*, moving the lever until the alignment and click seemed simultaneous, I stopped the motor, brought the contact-point to exact position at the mercury cup, and read the position of the pointer. Then, moving the finger-lever to the other side ('A' if the first position was 'B' and *vice versa*), I shifted the pointer in the opposite direction from the former shift; *i. e.*, again *towards* the fixed mark; started the motor, and proceeded as before. In this way I carried through the set, without any bias except from my

knowledge of the results. As a matter of fact, I was unable to recall on one day what my determinations had been on the day before; even what their general trend had been.

The starting position did not seem to vary within as wide limits as those used by Burrow when I was subject (75° to 45° from the fixed mark), ranging *perhaps* 35° to 60° from the fixed mark; but of course I have no data on the point.

Table I.

Rate 1.25 Secs. per Rotation. $1° = 3.47\sigma$.

Series.	'A.'	m.v.	'B.'	m.v.	Average.
1	+3.7	4.10	−7.3	4.56	−1.80
2	−1.2	3.92	−2.7	6.66	−1.95
3	+0.1	3.92	+1.8	7.36	+0.95
4	+4.4	6.28	+1.2	9.56	+2.80
5	−1.4	13.92	−0.1	3.9	−0.75
Averages	+1.12		−1.42		−0.15
σ	+3.88		−4.92		−0.52

Table II.

Rate 1.75 Secs. per Rotation. $1° = 4.86\sigma$.

Series.	'A.'	m.v.	'B.'	m.v.	Average.
6	+0.6	2.2	+0.8	2.04	+0.7
7	+1.0	1.8	−1.0	3.60	0.
8	−1.9	5.12	−0.4	2.60	−1.15
9	+0.4	4.4	+0.7	3.96	+0.55
10	+0.7	2.3	+0.2	2.56	+0.45
Averages	+0.16		+0.06		+0.11
σ	+0.77		+0.29		+0.53

Table III.

Rate 0.75 Sec. per Rotation. $1° = 2.08\sigma$.

Series.	'A.'	m.v.	'B.'	m.v.	Average.
11	−1.7	4.05	−4.8	5.84	−3.25
12	+2.7	9.20	+6.5	7.0	+4.6
13	+1.3	4.16	−3.5	7.12	−1.1
14	−2.6	9.00	−0.6	10.08	−1.6
15	−3.6	7.32	−1.6	5.2	−2.6
Averages	−0.78		−0.8		−0.79
σ	−1.62		−1.66		−1.64

The results of these experiments are given in Tables I., II. and III., in which the average displacement for 'A' and 'B,'

the m.v., and the average for 'A' and 'B' together are given for each day (each set of 20 determinations). The results are given in degrees, and the averages of the columns (of all the A's, of all the B's, and of all of both together) in *sigma* also.

The average total displacement for each rate is practically negligible; a trifle over one half *sigma* for the 1.75 rate, and the 1.25 rate, and a trifle over one *sigma* for the 0.75 rate. Compare this with my determination in Burrow's experiments (subject I., Table I., Sound: p. 31 of Burrow), which gave the following errors, expressed in *sigma*.

Rate.	'A.'	'B.'	Total.
0.75	−23.3	−22.9	−23.1
1.25	−46.0	−51.2	−48.6
1.75	−36.2	−45.1	−40.6

The sophistication due to my current knowledge of my individual errors *may* have been consequential, although there is no indication thereof in the results: there is no progressive change either in kind or amount of error from series 1 to 5, 6 to 10, or 11 to 15. To exclude all knowledge of results it was necessary to conduct experiments in a different manner and this I proceeded to do, obtaining the results given in Table IV. by the technique described below. Removing the circular 'dial' with the single mark, I substituted the 'dial' with uniform scale of 5 degree intervals, I shifted the cardboard disc without observing the amount of the shift by giving it a jerk in one direction, a jerk in the opposite direction, and then a third jerk in the first direction. Then, standing, with my gaze turned to one side, I shifted the finger lever to some position without noting it, and started the motor. After regular rotation was established, I determined the position of the pointer at which the click seemed to occur. I then read off the position of this point on the dial: and then the position of the finger lever as given by the brass scale, and recorded both. Then, after shifting the finger lever some distance inattentively, without stopping the motor, I proceeded to judge the new point of entry of the click. In this way I proceeded until twenty determinations had been made, after which I stopped the machine, set the finger lever at zero on its scale, and wrote down the corresponding reading for the pointer

on the dial. The next day I shifted the cardboard disc again, and proceeded as before. After completing the set of experiments, I computed the error in each case from the data. Thus I was enabled to obtain determinations without bias; at least I did not know during the series what sort of errors, or how large errors, I was making.

The direction of approach, which Burrow (and also Klemm[1]) have shown to be influential, I controlled by deliberately fixing my attention on a point either prior to or subsequent to the 'apparent region of simultaneity' as shown by the first few revolutions of the disc, and with further revolutions moved my fixation progressively from that point until satisfied. If my first determination was from a point in advance of my final fixation point (labeled 'prior' in the tables) the next was from a 'subsequent' point and so on alternately.

TABLE IV.

RATE 1.25.

Series.	Subsequent.	m.v.	Prior.	m.v.	Average.
16	+6.30	3.92	−2.75	4.6	+1.8
17	+3.45	2.91	−5.8	3.95	−1.17
18	+0.1	2.25	−7.3	3.4	−3.6
19	+0.42	3.6	−7.85	1.96	−3.72
20	+1.8	2.89	-4.5	8.83	−1.35
Averages	+2.42		− 5.64		−1.61
σ	+8.39		−19.57		−5.58

The greater irregularity of the results of this experiment as compared with the results of the first set is perhaps due in part to the heavy threads on the netting at 8 cm. intervals. For the first set the single fixed mark was midway between two of these heavy threads, and so they produced no disturbing effects; but in the second set, as the final index position at the time of the discrete stimulus varied around the dial, the positions emphasized by the threads were possibly influential.

Reverting now to Burrow's method, using the dial with a single fixed mark, I made another set of experiments, starting

[1] Klemm, O., 'Versuche mit dem Komplicationspendel nach der Methode der Selbsteinstellung,' 1907, *Psychol. Studien*, II., pp. 324–357.

in the determinations from 'A' and 'B' alternately as in the first set, but keeping the disk in a fixed position with regard to the contact arm. This relative position of the disk and arm was not noted until after the series was completed, and my readings were taken exclusively from the brass scale of the finger-lever.[1] Results of this set of experiments are given in Table V.

TABLE V.

RATE 1.75.

Series.	'A.'	m.v.	'B.'	m.v.	Average.
21	−0.37	3.65	−8.05	3.6	−4.21
22	+3.65	2.58	−2.77	2.85	+0.44
23	+2.10	3.68	−3.77	4.54	−0.83
24	+1.52	3.53	−3.40	3.55	−0.93
25	+7.50	3.05	+2.1	2.58	+4.8
Averages	+2.88	3.29	−3.17	3.42	−0.14
σ	+13.99		−15.40		−0.68

The remarkable thing about this set is that while the first series shows a rather large negative error and the last a rather large positive error the whole set averages less than 1 *sigma* constant error. It would have been interesting to take more series, to see if the positive error persisted, but of course I did not know the nature of the results until later, when I worked them over.

The averages are really modified by several unusual negative errors (one of 19°) in series 21, and several exceptional positive errors in 'A' of series 25. My notes written at the time of the experiments specify the set as especially difficult and unsatisfactory, fixation being hard to keep, so that it is probable that judgments of a type different from that of exact fixation crept into these two series. Concerning the formation of averages I shall have something to say later.

To see what my error would be with reversion to the natural fixation method I had employed in Burrow's experiments, I removed the net and took a fourth set of determinations, manipulating the apparatus practically as in the third set, *i. e.*, reading from the brass scale; and using the easiest rate, namely, 1.25.

[1] Thus, in this experiment, as in the preceding one, I did not know what errors I was making.

On the first day my tendency was to let the pointer blur, exactly as when using the net fixation. I allowed a week to go by without further experiment, and then took up the work again, when, without any effort except the attempt to get satisfactory judgments, the pointer came out distinctly at the apparent moment of the click. I proceeded then with the series, the results of which are given in Table VI.

TABLE VI.

NATURAL FIXATION. RATE 1.25.

Series.	'A.'	m.v.	'B.'	m.v.	Average.
26	+15.12	3.26	+10.72	3.82	+12.92
27	+14.67	2.06	+ 7.17	2.66	+10.92
28	+17.05	1.97	+12.90	5.32	+14.97
29	+13.42	4.92	+ 7.67	4.03	+10.55
30	+13.2	2.56	+ 5.85	3.54	+ 9.57
Averages	+14.69		+ 8.86		+11.78
σ	+50.97		+30.74		+40.87

The results for the third set of experiments show a surprising change in the error as compared with my determinations when subject for Burrow. My errors without the net were originally negative: now they fall on the positive side. This positiving tendency had shown in the last series given in Table V. which indicates the correctness of my surmise that some judgments by the natural fixation method had crept into that series.

II. Experiments on Other Subjects.

My experiments show quite clearly that with adequate fixation the average displacements in the complication experiment and similar experiments becomes practically inconsiderable. The netting simply gave me the means of knowing, and consequently of guarding against a blurring of a stationary object,[1] which was not so easily noted with the solid black fixation mark.

In October and November, 1909, I repeated the experiment on Dr. Watson, who had not previously served as subject with the complication apparatus or any similar device. He was aware of the results of my experiment on myself, and of the trend of

[1] On this point, however, see Dodge, R., 'An Experimental Study of Visual Fixation,' 1907, PSYCHOL. REV., PSYCHOL. MONOGRAPHS, Vol. VIII., p. 55.

my conclusions, and therefore was from the start observant of his fixation. His effort was explicitly to fixate the stationary mark as accurately as possible, and in this effort he soon succeeded admirably.

In the first experiments, without the net, I obtained from Dr. Watson each day twenty-four judgments, twelve with 'A' approach, and twelve with 'B' approach. For each side there were three starting positions of the lever, viz., 40, 50 and 70 degrees from the center of scale; and four starting points for the discrete stimulus with regard to the pointer position, viz., 30, 40, 50 and 60 degrees from the stationary mark. The twelve combinations of these variables were given according to a fixed scheme, so that the extents of movement of the lever, and the positions in which it was stopped, seemed to the subject entirely haphazard.

When we came to experiments with the net, the subject found the eye-strain severe, and was unable to complete twenty-four judgments at one session. The number was accordingly reduced, as indicated in Table VII., which gives the results of these experiments with Dr. Watson. The net was used for series 10, 11, and 12 only.

TABLE VII.

SUBJECT W. RATE 1.25.

Series.	No.	'A.'	m.v.	'B.'	m.v.	Average.	σ
1	24	—8.66	3.9	—25.75	2.4	—17.20	—59.68
2	24	—5.08	5.8	—13.16	7.5	— 9.12	—31.64
3	24	—2.91	3.7	— 4.75	5.0	— 3.83	—13.29
4	24	+6.25	3.9	+ 2.16	5.3	+ 4.20	+14.57
5	24	+4.33	1.7	+ 1.75	2.9	+ 3.04	+10.54
6	24	+3.41	1.9	— 2.25	2.7	+ 0.58	+ 2.01
7	24	+0.16	3.8	+ 0.41	3.5	+ 0.29	+ 1.00
8	24	+5.66	2.2	+ 2.33	3.8	+ 4.00	+13.88
9	24	+2.41	3.1	+ 1.16	2.9	+ 1.79	+ 6.21
10	22	—1.90	1.9	+ 0.63	2.3	— 0.63	— 2.18
11	18	—1.00	2.6	+ 3.11	3.6	+ 1.05	+ 3.65
12	12	—1.83	2.8	+ 2.16	1.7	—0.16	+ 0.54

The results of the experiments with subject *W* are given in degrees, because the spatial measure of the displacement is really more significant than the temporal measure, as we shall see later. For convenience, however, the last column gives

the averages of each series in *sigma*. The second column gives the number of determinations in each series, half being 'A' and half 'B.' Series 1 to 5 show a rapid reduction of an originally negative error, and then the change to positive, as described by Angell and Pierce.[1] From the sixth series on, the subject expressed himself as getting very satisfactory fixation, except in the eighth series, where he said he 'couldn't tell what he was doing.' The results agree with the introspective report. If we average the averages for series 6, 7, 9, 10, 11 and 12, we find a displacement of + 0.58 degree, or + 2.01 *sigma*. The net made practically no difference in this set of experiments, except to add to the eye strain of the subject. Not only was he subjectively certain that his fixation at the moment of the click was as steady without the net as with it, but the fact that the pointer blurred in exactly the same way in the one case as in the other proved that his conviction was well founded.

I next carried on some experiments with the subject who is designated subject III. in Burrow's report, and to whom I shall refer by that title hereafter. His errors had always been positive, ranging from 17 to 33 degrees. I subjected him to the experiment with the net, with instructions to fixate as steadily as possible. On the first two days he professed inability to maintain the fixation, or at least lack of confidence in his fixation, and the results were about as formerly. On the third and fourth days his fixation was more satisfactory subjectively, and the following series were obtained (Table VIII.). This subject

TABLE VIII.

SUBJECT III. RATE 1.25. VALUES IN DEGREES.

	Series 1.			*Series 2.*		
	'A.'		'B.'	'A.'		'B.'
	0		+8	+ 6		—10
	—2		+3	+ 9		+17
	—4		+6	— 2		+ 3
	—2		—3	+ 2		— 1
	+4		+8	— 3		— 5
	+8		—2	+16		+ 8
Average	+0.66	+2.0	+3.33	+ 4.66	+4.16	+ 3.66

[1] Angell, J. R., and Pierce, A. H., 'Experimental Research upon the Phenomena of Attention,' 1892, *Am. Jour. of Psychol.*, IV., 528–541.

found the eye strain severe, and the two series given were as long as could be carried through on these two days. The table gives the actual determinations, six 'A' and six 'B' on each of the two days.

It is evident that there are two classes of judgments represented in the second series above presented; one corresponding to the former results of Burrow's experiments with this subject, and one corresponding to the results obtained by subject *W* and myself with exact fixation. Leaving out the + 16 and + 17 from this series gives averages of + 2.4 and + 1.0, and a combined average of + 1.7, agreeing practically with the first series. Compared with the former errors made by this subject — averages from 17 to 33 degrees according to Burrow — these series are significant.[1]

Burrow's subject V. and four other subjects, who were entirely inexperienced, were tested briefly on the experiment. First, they were allowed to make judgments with natural fixation, and all made large errors — over fifteen degrees at the 1.25 rate in all cases, for the 'green' subjects, two of them being of the 'negative' type and two of the 'positive.' Subject V. made errors of slightly less magnitude (see Table I., Sound, in Burrow's paper). On being interrogated as to the perception of the pointer at the critical moment, care being exercised not to prejudice their answers, they all described a more or less definite impression, in cases when the judgment was of simultaneity and was satisfactory subjectively. In the cases of S1 and S3 slight rhythmic twitches of the eyelids were observable, following the rhythm of the click, and in the case of S2 slight *recovery* movements of the eye (movements in the direction opposite to that of the pointer movement) about every third click.

After these series by the natural method, the net was placed over the disc and stationary mark, and the subject was instructed as to the importance of exact fixation. All subjects now noticed

[1] All judgments by this subject which give an error of over 5 to 7 degrees are probably by the natural fixation method. After setting the stimulus with a larger error, the subject was able to correct it to within the above limits by squinting. It is characteristic of the experiment that if the fixation is lost during one or two rotations, it is usually lost for the remainder of the determination.

the lack of a clear impression of the pointer, and the judgments became more irregular, as well as more difficult.

A fifth 'green' subject, S5, found the pointer a mere blur at the critical moment, from the beginning of the experiment, and the net made no difference subjectively nor objectively. Her judgments, after the first half dozen, were distributed through the interval from $-5°$ to $+5°$.

A sixth 'green' subject, S6, squinted through lids almost completely closed, and made remarkably good judgments from the first; better than S5. I tried this method, but had no success, as the muscular success effort was too distracting. Subject V. tried it quite successfully, however, as did subject III. As to the physiological effects of squinting, I can make no inferences.

III. The Types of Judgment.

Exclusive of the judgments made by actually pursuing the pointer through some considerable portion of its circuit, we are forced to conclude that there are two distinct types of judgment, and these comprise two subtypes.

1. *The Exact Fixation Type.* — In judgments of this type the eye is practically at rest at the critical moment, although it is not necessarily stationary for any considerable length of time. (*a*) Spatial subtype: In these cases the judgment is specifically as to the approximate position of the pointer at the moment of the discrete stimulus. There is no time-judgment; the moment of the click and the moment of the alignment are not located in any series, nor with regard to each other. The sound sensation 'strikes out' in consciousness the visual sensation simultaneous with it, with an accuracy far in excess of any possible temporal discrimination. The pointer is however not a distinct positional (spatially) image at the moment of the click, but a blur extending over an area greater than the normal retinal image of the pointer. Theoretically, the judgment should be satisfactory when the front edge of the blur is approximately abreast of the stationary mark, but practically we should not expect a subject to get down to such accurate discriminations. Individual predilections for certain characteristic

relations of blur and stationary mark may also be expected to produce slight constant errors.

Spatial exact-fixation judgments are difficult for most subjects; the strain in attempting to produce eye-steadiness at the proper moments is considerable, especially in the presence of a tendency to react rhythmically, as described below, and the evaluation of the relative position of blur and fixed mark requires intense concentration. In any case, a shift of several degrees in the position of the pointer may be made without seriously injuring the apparent simultaneity.

(*b*) Temporal subtype: In these cases, the judgment is as to the occurrence or non-occurrence of a time-interval between the discrete stimulus and the approximate alignment of the pointer with the stationary mark. Judgments of this type do occur, as judgments of non-simultaneity, when the discrete stimulus is temporarily remote from the discrete stimulus, as at the beginning of a determination by Burrow's method. But the irresistible tendency to note the position of the pointer at the moment of the discrete stimulus, if it is spatially near, prevents this form of judgment from entering effectively into the final determination; at least my own introspection and that of other subjects leads me to this conviction, although it must be admitted that introspection on these points is not very reliable.

That the temporal form of judgment allows greater latitude for error than does the spatial is clearly brought out by some experiments which I will now describe. These experiments also exhibit the general symmetry of the errors about the point of actual equality, *i. e.*, the lack of a constant error.

The disc bearing the index-hand was removed from Burrow's apparatus, and replaced by a disc bearing a radial slot 4 mm. wide which extended from the edge 50 mm. inwards. The semi-circular dial was replaced by another which was slightly below the level of the disc, and extended under it by more than the depth of the slot. On this dial was placed, on the middle radius, a black strip 45 mm. long, 2 mm. wide at the inner end, and 5 mm. wide at the outer end. The strip was so placed that the wide end projected slightly beyond the edge of the disc into the view of the subject: the re-

mainder of the strip was consequently exposed only during the passage of the slot across it. An electric light placed at the outer edge of the dial, but shaded from the eyes of the subject, threw a good light on the dial beneath the disc. This arrangement provided the conditions for a pure temporal judgment, the black strip appearing before, at, or after the click of the telephone, and the temporal position admitting of variation by the moving of the finger-lever as in the previous work. These conditions are much more satisfactory than with the combination of a flash of light with a sound, for with the black-exposure apparatus the duration of the image of the black mark is positively terminated by the portion of the white disc following the slot.

All the subjects employed on this experiment, except S5, made large errors with this apparatus; much larger than by the natural fixation method with the pointer-disc; and both positive and negative. But actual simultaneity never failed to be perceived as such. I therefore proceeded to find how far the click could be separated from the exposure, in each direction, without the subject losing the perception of simultaneity. Setting the disc for simultaneity with the finger lever at zero on the brass scale, I instructed the subject to move the lever to the right until the click and the exposure appeared non-simultaneous, and then back until simultaneity was just reached. This being done, and the position recorded, the finger lever was returned to zero, and the subject was instructed to repeat the determination in the other direction.

With the exception of Dr. Watson, the limits of the subjects were symmetrical with respect to the actual simultaneity point, and very regular, *i. e.*, were repeated in succeeding determinations almost exactly. The limits for subjects II. and V. were 20–22 degrees in each direction; for myself, 10–12 degrees, for S6, 20–21 degrees, and for subject S5, 3–5 degrees. Dr. Watson proved an exception to the rule as regards symmetry and regularity, his limits running from 20 to 30 degrees with exposure-click and from 10 to 16 with click-exposure. He reported however that the observation was easier with click-exposure than with the other order.

The general symmetry of the indifference-zone in this pure time experiment agrees with the results obtained by Wundt and by Geiger[1] with the complication-clock when they covered the dial except along one radius, at which the pointer was exposed in passing. But the experiment has more importance than Geiger is disposed to admit, since it shows some results obtained by Weyer[2] in measuring the time-threshold between visual and auditory sensations have no bearing whatsoever on the complication experiment, although a mistaken impression as to the significance of these and similar results seems to have been of strong influence on the apperception or 'prior entry'[3] theory of the displacement in the complication experiment.

Weyer found that the time-threshold between a visual and an auditory stimulus depended on which stimulus preceded the other, and on which the attention was fastened predominantly. The threshold was much greater when the visual stimulus preceded the auditory than when the reverse order occurred, other conditions remaining practically uniform. If, when the light came first, the attentive emphasis was on it, the threshold was only about half as great as when the attention was fastened on the sound. When, however, the light came last, attention predominantly to it increased the threshold.

Since the attention to the first member of the pair shortens the time-threshold, and attention to the second lengthens the threshold, exactly as if the interval for any given separation were subjectively lengthened in the first case and shortened in the second, it is easy to assume that the sensation strongly attended to gets more quickly apperceived than the other, and hence the subjective lengthening or shortening of the interval actually occurs. From this point, application of the results to the explanation of the complication experiment phenomena is a simple matter. The prevalent negative error becomes the expression of the greater readiness of 'apperception' to seize on auditory stimuli sensations rather than visual and especial attention to the visual factor, either by voluntary effort or through

[1] Geiger, M., 'Neue Complicationsversuche,' *Philos. Studien*, XVIII., 398.

[2] Weyer, E. M., 1900, *Philos. Studien*, XV., 67–138.

[3] Titchener, E. B., 'The Psychology of Feeling and Attention,' Lecture VII.

some added interest, may overcome this apperception-differential, and produce a zero, or even a positive error.

As a matter of fact, conditions in the spark-snap experiments of Weyer, in which a flash of light appears on a dark background, are quite different from those in the complication-experiment, and from those in the combination of click with exposure of a black strip, as described above. In this latter case, it makes no difference whether the auditory stimulus precedes the visual or *vice versa*, and shifting the attention seems to produce no effect so long as enough attention is given to both to make the judgment possible. Moreover, Weyer found that the time threshold for two successive flashes was from 25.3σ to 49.7σ for flicker, and from 42.6σ to 105.9σ for complete separation, whereas with the exposure apparatus described above the black strip appears sharply separating the preceding from the following white, when the extreme duration of the exposure is less than 5σ!

The greater threshold for light-sound than for sound-light under Weyer's conditions depends of course on the greater persistence of the visual sensation.[1] As for his findings with regard to attention, we may summarize them as follows: In a sequence of two sensations, one of which has a longer duration than the other, attention to the longer emphasizes its beginning if it is the first, and its latter portion or approximate ending if it comes last. That it makes any difference whether the two sensations are of the same or of different senses, no one has yet taken the trouble to find out.

The exact fixation type of judgment, in either the spatial or temporal form, is characterized by the fact that actual simultaneity is never perceived as non-simultaneity. It shows therefore a genuine indifference zone, in which lies the point of actual simultaneity, and about which, in the temporal form, it is symmetrical. The reaction or natural fixation type of judgment, we shall see, shows a *quasi* indifference-zone, with the point of actual simultaneity lying in general outside of it.

2. *The Natural Fixation or Rhythmic Reaction Type.*— When the eye is not obviously following the pointer, and yet a

[1] Wundt. *Physiol. Psychol.*, 5th ed., Vol. III., p. 65.

tolerable image of the pointer is obtained at the critical moment, there must be some sort of activity which obtains this image, since with the optical apparatus in an actually static condition the pointer would be reduced to a blur at all but the slowest rates. We are justified in speaking of a reaction as the producing cause, although we may not be able to specify the exact nature of this reaction. The activity in question may be, and it seems probable that it is, a slight movement of the eye in the direction of the pointer, at the critical moment. This reaction is an involuntary movement, guided by the rhythm of the discrete stimulus and the movement of the pointer. That it may be very slight, and yet produce its effect in dissociating the pointer from the blur, may be shown by the following experiment.

A disc, composed of eight or more equal sectors alternately black and white is rotated behind a double cross made of four narrow black strips, enclosing a small square at the center of the disc. The strips are brought as close as possible to the disc without rubbing. The speed of rotation is such that the sectors just blur. On allowing the eyes to wander across the disc, a portion of a white sector will occasionally flash out, and sometimes a sector will appear sharply cut across by one of the black strips. The best results are obtained by having fixation marks at the right and at the left of the disc, and changing the gaze from one to the other without attending to the eye-movement.

The appearance of the sector cut cross by the strip, can be explained only as due to the simultaneous impression of the two on the retina. If the retinal impressions were received in succession on the same area, the image of the white sector would cut across the black strip, instead of being interrupted by it. It is evident therefore that the angular distance traveled by the eye while receiving the image of the pointer effectively, is very small, for the simultaneously received impression of the stationary strip does not blur. That the impression of the sector is due to a momentary correspondence of eye movement and movement of the seen portion of the sector, and not to an alternation of periods of visual sensitivity with periods of visual anesthesia,

is apparently proved by the fact that the sector seen is always one which is moving in the direction of the eye-sweep; on the bottom of the disc therefore for one direction of eye movement, on the top for the other: and by the fact that when the sector flashes out no portion of the double cross is visible except the portion that happens to lie across the sector.

How slight an amplitude of movement of the eye would produce the flashing out of the sector cannot be estimated with any closeness. I made some rough experiments by dropping the disc, the eye remaining fixated on an object placed vertically above the center of the disc. With the eye three meters from the disc, a drop of 0.7 cm. always brought out the inner half of the sectors on the upward-moving side of the disc (the outer portion was moving too fast to be brought nearly to rest by the slight velocity of the drop). Since this drop is equivalent to a movement of the anterior surface of the eye of less than 3/50 mm., and since the latter part of the drop of the disc was alone effective (the velocity in the earlier part being too low), it is evident that a movement of the eye not only too slight to be seen by another observer, but even too slight to be recorded by any of the photometric methods now in use, would be sufficient to bring out the image of a moving object at moderate rate, if the eye movement was in the direction of the object's movement, and at a rate near that of the object.

Whether the reaction by which the cerebro-retinal image at a given moment is differentiated from the images preceding and following, is an eye-movement, or some other activity, may be left an open question. I shall treat it for the present as if it were an eye-reaction, since all analogies point in that direction.

After the rhythm of the discrete stimulus and of the pointer-rotation is definitely established, the subject of the complication-experiment who uses natural fixation notes only one position of the pointer in each round, the reaction being made either with the discrete stimulus, or, in exceptional cases, at the alignment regardless of the discrete stimulus. If the pointer is caught approximately at the alignment with the stationary mark the click will seem simultaneous with that position of the pointer, if it does not come too long before or after; in other words, the

judgment is in so far on a temporal basis, although the error may be regulated by non-temporal factors.

If the pointer is caught at some other than the fixated point, with no appreciable time-interval between this catching and the discrete stimulus, this occurrence is taken as a sign of non-simultaneity at the fixated position, and accordingly, by Burrow's method, the temporal position of the discrete stimulus is altered, or, by the classic method, the fixation point is changed.

Whenever the reaction occurs within the limits of the time threshold before or after the discrete stimulus, which limits are variable, but may be safely said to be less than one hundred *sigma* in ordinary cases, the discrete stimulus will seem to occur at whatever point in the path of the pointer happens to be that at which it is caught by the reaction. The determinant of the error will therefore be the lack of coincidence of the reaction with the discrete stimulus.

Voluntary rhythmic reactions, *i. e.*, reactions to a stimulus repeated in regular rhythm, may be of two sorts. The reactor may attempt to make the reaction, or a definite part of the reaction, synchronous with the stimulus, or he may attempt to make it follow or precede the stimulus.

It is generally supposed that synchronizing rhythmic reactions may be made with surprising accuracy, although subject to some irregular variation. For the purpose of observing such reactions I modified my apparatus in a new way. I suspended a helium tube just above the fixed mark on the scale (in practically the position of the Geissler tube in Burrow's experiment with light), so that when the tube lit up it illuminated both stationary mark and the adjacent portion of the disc. This tube was connected in the secondary circuit of a large induction-coil, and a break key was inserted in the primary circuit. The room was darkened so that as the subject reacted by depressing the key, the position of the pointer with regard to the stationary mark was distinctly visible to the experimenter at each reaction. The telephone-click, as described above, was employed for the stimulus, the telephone being placed close to the ear of the subject as he sat at a table with the reaction key before him. The disc was so adjusted that the click came exactly at the

alignment of pointer and stationary mark, so that the error in the reaction could be read off directly in degrees.

Nearly all of the subjects required a half hour of practice before they succeeded in reacting to the rhythmic stimulus in a manner satisfactory to themselves. Four subjects, who regularly made negative errors in the complication experiment, anticipated almost exclusively in the reaction. The anticipation, in cases where the reaction and stimulus seemed to the subject simultaneous, was in the majority of cases within 20 degrees of actual simultaneity at the 1.25 rate, but larger errors were frequently perfectly satisfactory to the subject. Very seldom did these subjects hit within 5 degrees of simultaneity, and only occasionally was the reaction delayed.

Subject III. always began a series of reactions with five or six delayed reactions, changing then to anticipatory, and reverting sporadically to the delayed type. Taking his series as wholes, the delayed reactions numbered about one to two anticipatory reactions, the magnitude of error being not much different from that with the four subjects just mentioned. The errors of this subject in the complication experiment, it will be remembered, were positive.

Dr. Watson anticipated as a rule, and the errors in some of his series came under 20 degrees at the 1.25 rate. In some series, however, the reaction was premature by from sixty to fifty degrees, and yet seemed to him to synchronize perfectly with the click. Occasionally the subject delayed, but the ratio of delayed to anticipatory reactions was not higher than one to ten. Within series of from twenty-five to forty reactions, Dr. Watson was more uniform than the other subjects; frequently I counted eight to ten reactions in succession which did not vary more than two or three degrees from each other.

I myself usually anticipated, but on some days delayed almost exclusively. Judging by the reports of the various persons who observed my reactions, my errors were about like those of the first four subjects mentioned above.

Most of this work was done with the one rate of 1.25 sec. per rotation. I tried other rates, from 2.0 secs. to 0.71 sec., but found no essential difference in any case; the amounts of

the errors in degrees changed perceptibly with the rate, but as nearly as I could estimate the amount in time remained the same. The key used broke contact at the bottom of the stroke; a key which broke contact at the beginning of the movement gave exactly the same results with four subjects on whom I tried it.[1]

After satisfying myself as to the subjects' performances in attempted synchronizing reactions, I instructed them to fall in behind the click; to wait on the stimulus as it were, but to attempt to follow it with the least possible intervening interval.

After a little practice, all of the subjects except two found the follow-reaction easier and more satisfactory than the synchronizing, and their errors were actually much less, being uniformly delays. Of the two subjects who had difficulty with the follow-reactions, one made smaller errors and the other larger than in the synchronizing reaction.

The results of these rough preliminary experiments agree with the statement of Scripture[2] that almost all subjects anticipate in rhythmic reactions, and agree also with the usual negative error with the complication experiment. The greater ease of the delayed reaction throws light on the tendency of the error to become positive after habituation to the experiment.[3] Certain reactions giving the subject more satisfactory judgments than do others, he gradually falls more and more into the habit of that way of reacting.

More extensive investigations into the rhythmic reaction may show an influence of rate corresponding to the effect of rate upon the complication judgments. Certain rates will undoubtedly be found which will call forth the maximum of accu-

[1] The effects of practice with and without orientation offer a series of problems whose solutions are of the highest importance. Certain of these problems it is my intention to take up in the near future, with apparatus recording the errors instead of merely making them visible. In this connection a brief experiment with the apparatus described, with a subject experienced in astronomical observation, and hence with preceding oriented practice in the following of rhythmic stimuli, is suggestive. This subject, at the rates 1.0 and 0.6 made errors ranging from 0 to 5 degrees, about as many delayed as anticipatory. On the complication experiment, his errors were of approximately the same sort: positive and negative indifferently up to 5 degrees.

[2] Scripture, E. W., 'Observations on Rhythmic Action,' 1899, *Yale Psychological Studies* VIII., p. 103.

[3] Angell and Pierce, *op. cit.*, p. 534. Geiger, *op. cit.*, pp. 358–364.

racy for any given subject, and possibly the error will be found in other respects to depend somewhat on the rhythm.

The reaction in the complication experiment, especially after fair habituation to the conditions, is normally to the discrete stimulus, although the rhythmic movement of the pointer is a disturbing factor, especially at first. The limits of error are therefore the maximal anticipation and delay in the reaction, since these are in general less than the least observable time differences between reaction and stimulus. This latter is shown by the fact that normal error in the rhythmic finger reaction is less than the errors which occasionally occur without being noticed, and also by the fact that the limit of error in the pure time-judgment experiment (the black strip exposure experiment) is for all but one subject greater than the errors of the same subject in the complication-experiment.

If, after the subject in the complication-experiment has found a position for the discrete stimulus which gives a satisfactory judgment of simultaneity, but with error, the stimulus is moved *nearer* to the actual alignment, the judgments on this new position will in general not be of simultaneity, because the reactions will not now catch the pointer at alignment. Some slight variation is possible, because the pointer is never caught with absolute sharpness.

Reactions to the pointer, instead of to the discrete stimulus, are possible, but are irregular and unsatisfactory, as each variation in the error changes the rhythm. With the finger reaction device described above the subject, if placed in the normal position before the apparatus for the complication-judgment, could flash the pointer out repeatedly near the stationary mark, without the aid of the click. The errors were of the subjects' usual types, but very erratic. In the complication-experiment judgments by reactions to the rhythmic passage of the pointer would have as limits of error the maximum imperceptible time between the reaction and the discrete stimulus. In the case of a judgment giving error the discrete stimulus might be placed nearer to or at the actual alignment and still the appearance of simultaneity be preserved. In general, as above stated, this substitution cannot be made in the case of a determination by

the natural fixation method; which shows that the pointer-reaction practically does not figure in the final judgments by this method. Yet in Burrow's method such reactions do occur at the start of a determination, because then the pointer is too far from the fixation mark to be clearly seen at the moment of the discrete stimulus. This is shown by the introspection of several subjects on the judgment directly; that the judgment was of time elapsed between the discrete stimulus and the alignment; and also by the practically unanimous report that large shifts of the lever produced relatively slight effects at first — shortening a long time interval produces an effect disproportionately small. These judgments apparently did not occur after the click came near to the alignment, and hence Burrow's attempt to get determinations by the temporal method alone [1] proved a failure.

James' theory [2] of the time displacement; that it is due to the inability of attention to deal with the impression of the pointer and that of the discrete stimulus at the same time, because of which inability it attends to them in succession, and the time interval not being perceived, the succession is mistaken for simultaneity; might be modified to suit the reaction hypotheses. We might suppose that the subject is unable to attend to other than the visual stimulus at the time of the reaction, hence requires a succession such that he can attend to the discrete stimulus just after or just before the reaction. Judgments without error would be supposed to be due to the use of some other than the reaction method of observation. This explanation of the time-displacement is excluded by the results of the following experiment.

Allow a subject to make a determination in the complication-experiment, using the natural fixation method. If he makes a considerable error, arrange a second contact point, to produce a second click, exactly at the alignment at which the subject thinks the first click comes. Call the first click C_1 and the second C_2. Now both clicks being heard in succession, the subject, if his

[1] Burrow, *op. cit.*, pp. 54–55. I do not agree completely with Burrow's explanation of the 'spatial-temporal' distinction, and I do not think it corresponds to the 'naive-reflecting' classification of Geiger.

[2] James, W., *Principles of Psychology*, I., p. 415.

original error was positive, will judge that C_2 is correct, and C_1 is incorrect. Cut out C_1, and C_2 seems too late; cut out C_2, and C_1 seems correct. With both clicks (it is safe to conclude), the subject, reacting to the first click, catches the pointer at the alignment, whether the second click occurs or not. If the second click occurs, since it comes exactly at the perceived alignment, it is perceived as simultaneous therewith, and the first click seems therefore too soon. If C_2 alone is given, the subject usually reacts to that as he had previously to C_1, and therefore will not catch the pointer at the alignment.

With subjects making a negative error the experiment does not always succeed. Here C_2 comes before C_1, and there is a tendency to react to it. But if C_2 is cut out, by means of a key in the circuit, and the subject is allowed to observe for several rounds of the pointer, thus getting the rhythm of C_1 well established, and if C_2 is then suddenly thrown in, the subject will usually go on reacting to C_1 for a few rounds, getting C_2 therefore in simultaneity with the alignment.

If the double-click experiment is applied to a subject making an error by the exact fixation method, the second click will appear correct, and will remain so when the original one is cut out. This was notably the case with Dr. Watson, and sharply characterizes the exact fixation method. The test is however applicable only occasionally to subjects of this type, since the errors are usually too small to allow of the insertion of the contact for the second click at exact zero.

There can be no question, it seems from the above described experiment, that the subject is perfectly well able to attend to the discrete stimulus at the moment of the alignment. The source of the errors lies in his inability to react so that the pointer is caught at the moment of the stimulus. By the reaction method, as by the other methods, simultaneous impressions of the pointer and of the sound *are always perceived as simultaneous*, but impressions not simultaneous are also perceived as simultaneous, if the time discrepancy is not too great. The error is due to the fact that the subject, on account of his peculiarities of reaction, does not get an impression of the pointer at the moment of the discrete stimulus, but does get one just before or just after.

The apperception explanation of the time displacement, which assumes that impressions actually simultaneous seem non-simultaneous, is completely discredited by the above facts. Such an illusion has been shown to occur in the case of two stimulations of which one lasts longer than the other, when both begin at the same moment. In that case the illusion has a definite direction, not reversible, although the extent is variable. In other circumstances no illusion of time-displacement has been demonstrated. *In the complication experiment there is no illusion of time-displacement; the displacement is real, and the fallacious perception is of simultaneity.*

Some observations made with an artificial pupil seem significant although their exact bearing is not yet apparent. If the artificial pupil is moved to one side, the other side of the disc (*i. e.*, the right side, if the pupil is moved to the left) may be hidden. If the left side of disc and dial were hidden in this way; except for a narrow strip adjacent to the stationary mark, and to the pointer when in alignment with the mark; the experienced subjects (Dr. W., subject III., subject IV., and myself) made positive errors exclusively, while the 'green' subjects all made negative errors. (The pointer rotated from left to right in passing the mark; *i. e.*, clockwise.) With the right half of the field covered, the errors were for all cases the reverse of the errors with the left half of the field covered. Covering the field by a cardboard just above the level of the disc had no effect on the direction of the error in any case. The fact that in this latter case movements of the eye had practically no effect on the extent of the visual field, while with the artificial pupil a slight movement in one direction cut off the entire field, and movement in the other direction enlarged the field, may shed some light on the phenomenon.

3. *The Pointer-pursuit Method.* — Thus far we have assumed that the judgments in the classic work on the complication experiment from Wundt to Burrow were made by the natural fixation method. Burrow's subjects certainly employed this method. Wundt,[1] von Tschisch,[2] and Klemm give no

[1] Wundt, W., *op. cit.*, pp. 67–86.

[2] Von Tschisch, W., 'Ueber die Zeitverhältniss der Apperception einfacher und zusammengesetzer Vorstellungen,' 1885, *Philos. Studien*, II., pp. 603–634.

information as to their methods of observation; Pflaum[1] says that he followed the method of Wundt and von Tschisch. Angell and Pierce[2] always followed the pointer on its first round, and then proceeded to fixate the point so picked out. Geiger[3] found two classes of subjects, whose methods he calls the 'naïve' or 'pointer-observing' and the 'reflecting' or 'scale observing.' He admits that the two methods are not always clearly separable. In observing by the 'reflecting' method the subject practically used natural fixation after the first few rounds of the pointer; as to the fixation during the first rounds we are not accurately informed. By the 'naïve' method, the subject pursued the pointer for the first few rounds, and then employed natural fixation. Geiger's 'naïve' type therefore corresponds practically to the method of Angell and Pierce, although for purposes of argument Geiger classifies them as 'reflecting.'[4]

Since it is possible to follow the pointer at rates up to at least 1.0 sec., and perhaps at still faster rates, it is important to find out what sort of judgments result from this method, that we may know what effect these judgments may have had in the classic experiments.

Angell and Pierce[5] reported that judgments based on the first rotation (the pointer being followed during this rotation), gave invariably positive errors. I wished to check this observation, and also to test the explanation they offer; that the eye comes to a stop after the discrete stimulus, the error measuring the time required for this arrest.

On the shaft of a controlable motor I mounted a white cardboard disc of 28 cm. radius. In this disc I cut a radial slot 5 degrees wide and 15 cm. long, extending from a point 8 cm. from the center of the disc to a point 5 cm. from the edge. This slot was covered with white tissue paper pasted on the back of the disc. Behind this disc was placed a stationary disc of

[1] Pflaum, C. D., 'Neue Untersuchungen Ueber die Zeitverhältniss der Apperception einfacher Sinneseindrücke am Complicationspendel,' 1900, *Philos. Studien*, XV., pp. 139–148.

[2] Angell and Pierce, *op. cit.*, p. 533.

[3] Geiger, *op. cit.*, p. 374 *et seq.*

[4] Geiger, *op. cit.*, p. 384.

[5] Angell and Pierce, *op. cit.*, p. 534.

black cardboard, having in it a hole about 14 mm. in diameter, 18 cm. from the center. This disc slipped over the bearing of the motor-shaft, and could be turned to bring the hole in any radius, being held by clamps from the side and top when in position. Directly back of the hole was placed a 16 c.p. electric light. The revolution of the slotted disc therefore exposed progressively a roundish area of yellow light as the slot passed in front of the hole, the slot being much narrower than the hole. Perhaps better results would have been obtained had the tissue paper covered the hole instead of the slot.

In operation, the illumination on the front of the slotted disc was reduced to such intensity that the flash of the exposure of the hole was bright; the face of the disc was still plainly seen, and the slot appeared distinctly as a dark stripe. The subject was seated at a distance of six or eight feet in front of the disc.[1]

At rates of from 0.5 to 2.5 sec. per rotation of the disc, the subject if given no observation instructions, usually sees the flash as a practically *round* spot, *on the surface of the disc behind the slot;* sometimes as much as 90 degrees behind. The roundness of the spot shows conclusively that the eyes (or eye; the results are the same if one eye is used) are at rest at the moment of the exposure, or at least are not moving fast enough to distort the image; the movement at rate sufficient to obtain an image of the slot coming shortly after the exposure, as shown by the positive error. The reaction is in short delayed.

If the subject begins to reproduce the rhythm of the flash muscularly, the error (*i. e.*, the apparent lag of the spot behind the slot) becomes more irregular, but in general less, and may change into a negative error (*i. e.*, the flash may appear on the disc in front of the slot).

By careful fixation of a mark placed in front of the disc, the error may be practically destroyed. In this case the slot no longer appears distinctly. A narrow strip of black cardboard

[1] I have also used with good results a slotted light-gray disc with a round white spot pasted on a black disc, and a slotted dark-gray disc with a black spot pasted on a white disc. Either of these combinations will give good results, but the round hole illuminated from behind is more satisfactory, because the intensities are easier to adjust, and also because the other combinations must be placed more carefully with regard to the direction of illumination of the room.

supported in front of the disc, and extending radially from the position of the light spot makes a good fixation mark. Perhaps cross-lines would be better. For some reason fixation is easier than with the complication-disc, possibly because the slot is less prominent than the black pointer used in the other experiment.

So far the experiment is a simple and striking verification of Burrow's finding, of displacement with visual discrete stimulus, and also of my findings with regard to the effects of exact fixation. The most important results come out with pointer pursuit.

The subject can easily pursue the pointer (with reservation noted below), at rates covering a wide range. The light-spot now appears no longer round, but as a narrow streak, being in fact the illumination of a section of the slot. There is therefore no displacement of slot as regards the spot, but the illumination seems to be farther along in the path of the slot's rotation than it really is — there is a decided positive error. If a fixed mark is supported in front of the disc, on the radius of the light spot, as described above, the displacement is especially notable.

The explanation of this displacement is undoubtedly given correctly by Angell and Pierce. The eye comes to rest a certain length of time after the occurrence of the discrete stimulus, but the time interval is not noted, and hence the stimulus is assumed to have happened when the eye was in the position in which it comes to rest. In the case of the visual stimulus, the after-image of the flash assists the illusory effect, because it remains in the position in which the stimulus is supposed to have lain. But the conditions would be much the same without any visual after-image. The eye, during true pursuit movements is unable to orient itself, and must stop for that purpose; while moving objects are being pursued, stationary objects are not distinctly seen. In this particular the true pursuit movement differs essentially from the movement which brings out the sector in the experiment earlier described.

Sometimes, the eye, while the subject is attempting to pursue the pointer, comes to a stop before the flash. In this case the flash is *round;* a certain indication. If the eye stops while

the light is being exposed, the spot is D-shaped, the straight edge being in the rear; and if the eye is at rest at the beginning of the exposure, commencing to move before the end, the spot is D-shaped, but the straight edge is in front. In the last two cases, which occur infrequently, there is no displacement. In the case where the eye stops before the exposure, the displacement is in the negative direction, for obvious reasons. Apparently, the eye, if it stops at the exposure, or immediately before it, does not get in motion for some little time afterwards, so that positive errors in these cases are practically out of the question. Positive errors do occur, however, with round spot, while the subject is attempting to pursue the pointer, because the eye stops so long before the flash that the judgment becomes practically of the natural fixation type. In these cases, the subject usually realizes that he is not following the pointer accurately.

This tendency to follow the pointer inaccurately becomes stronger after the first one or two rounds. The first few rounds, the pointer is easily followed, and the slot-shaped spots and positive displacements obtain exclusively. Then, the tendency to run ahead and fixate becomes strong, and the round and D-shaped spots — principally round — begin to occur. The natural tendency is to cease the effort to pursue, and to fixate. Hence, I conclude that Geiger's 'naïve' subjects were of the same type as Angell and Pierce. We may safely conclude that all the classic observations were made by the natural fixation method, tempered in a few cases by preliminary observations by the pointer-pursuit method. How much influence on the final judgment is exercised by the preliminary observations is a question to be settled by further experiment.

IV. The Influence of Emphasized Points.

Wundt[1] and Geiger[2] noticed that if certain points in the path of the pointer are especially emphasized, as by the placing of marks, or simply by their being the top, bottom, etc., of the path, there is a tendency for the apparent point of entry of the discrete stimulus to be drawn towards such emphasized

[1] Wundt, *op. cit.*, p. 69.

[2] Geiger, *op. cit.*, pp. 384-394.

points. This observation has so far remained a mere brute fact, not provided for by any theory of the complication error, but it is readily accounted for after discriminating properly between types of observation.

By the exact-fixation method, the position of the pointer is indistinct, and it covers a small stretch of path rather than an approximate point. Hence, any fixed point within that stretch is apt to be selected as the locus. If such a point is brought out in any distinguishing way, it is much more liable to be selected, for there is no positive reason against the selection. Hence, in the experiments on Dr. Watson and myself, there shows a constant difference between the 'A' and the 'B' judgments. On the average, the correction will be stopped before it quite reaches perfection, for if there is a slight range of indifference for any setting, within which the emphasis of a point is effective, when the adjustment brings the actual alignment within this range it will be satisfactory.

The indifference-zone displayed in the results of Burrow's experiments owes its existence to a different cause. By the natural fixation method, the perceived position of the pointer is much more exact than by the exact fixation method. Hence the actual range of indifference in any given judgment is relatively small. The emphasis of any point operates here by modifying the delay or anticipation in the reactions on which the judgments are based. If the reaction catches the pointer a little before (or after), a point which is especially prominent, there is inevitably a feeling of prematureness (or lateness) and the succeeding reactions will be slightly more delayed, or less anticipatory (in the one case, and more anticipatory or less delayed in the other).

The distance of the starting point from the actual alignment was found by Burrow (see his Tables III. and IV.) to be very important; the greater the discrepancy at starting the more the error at the final judgment tended in the direction of that discrepancy, or the less it tended in the opposite direction. Evidently, the more distant the starting point from the final setting the more pronounced the effect in increasing or decreasing the anticipation or the delay. This operated in a rather relative

way, the contrast with the longer distances modifying the effect of the shorter. Thus, while the 'A' 75° starting point gives in all cases (except with subject II., for reasons explained by Burrow), a more positive value of the error than the 'B' 75° starting point; the relation is more often reversed than not by the 45° starting points.

The results of my experiment on Dr. Watson show a slight tendency in the same direction as the results in Burrow's tables. The averages for the four starting positions on each side, for 'A' and 'B,' of the series from 3 to 9 inclusive, are given in Table IX. below, in degrees.

Table IX.

Starting Positions.

Subject W. Series 3–9.

	'A.'	'B.'
30°	+2.09	+1.71
40°	+2.27	0.04
50°	+3.27	—1
60°	+3.38	—0.28

This tendency indicates pretty clearly what I had other occasions to suspect, namely, that certain of the series contained a few judgments by the natural fixation method. This takes us over to the matter of the composition of averages from judgments of different sorts.

V. The Making of Averages.

Although the previous statements with regard to the complication experiment have been based on averages, none of the experimenters has evidenced any concern as to the composition of these averages. Burrow is the only one who has presented the mean variations, and these are large enough to give food for thought. Pflaum, it is true, does give the number of positive and negative judgments, respectively, represented in each average. In Pflaum's results, practically every average is drawn from errors of both types, and in some cases there were two of one kind and one of the other! In other cases there were two of one kind and three of the other, and so on.

With the exceptions mentioned, the literature contains no

details concerning the formation of the averages except the number of determinations. And yet, it is very important to know, in cases where the number of determinations entering into the averages is adequate, whether a decrease (for example) in the magnitude of the average error means that the average *magnitude* of the negative errors decreased, or merely that the *number* of negative errors decreased. Such information is of especial value in cases of subjects who show a gradual change of the average error from negative to positive.

Table X. gives the number of positive and negative errors in each average of Table VI. (subject *W*), and also the average for the errors of each type separately. The figures show an irregular fluctuation in both positive and negative errors, but no progressive change in either positive or negative after the fourth day. In general, from the fifth day on the average magnitudes of positive and negative errors are nearly equal. Table XI. gives the positive and negative averages from my own judgments. In my case the fluctuations are more pronounced. Averages are given in degrees in both tables.

Table X.

Numbers and Averages of Positive and Negative Errors from Table VII. Subject W.

Series.	'A.'				'B.'			
	Positive.		Negative.		Positive.		Negative.	
	No.	Av.	No.	Av.	No.	Av.	No.	Av.
1	0	—	12	8.66	0	—	12	17.40
2	3	4.66	9	8.33	0	—	12	13.16
3	3	4.66	9	5.44	2	3.50	8	6.4
4	11	6.81	0	—	5	7.60	5	2.40
5	12	4.33	0	—	8	3.87	3	3.33
6	10	4.7	2	3.30	2	3.50	7	4.85
7	6	2.66	6	2.33	7	3.42	5	3.16
8	12	5.66	0	—	7	4.56	3	1.33
9	9	4.33	3	3.33	7	3.62	3	4.00
10	2	4.0	8	3.12	4	2.50	6	2.80
11	3	2.66	5	3.40	5	6.80	2	2.40
12	2	2.5	4	6.0	3	5.33	1	3.0

The series from 16 to 25 inclusive, of my results (from Table IV.) are listed in Table XI. with the 'subsequent' determinations under 'A' and 'prior' under 'B,' to which of course

they correspond, since the 'A' approach means that the fixation point was at first *after* the actual point at which the discrete stimulus occurred.

TABLE XI.

NUMBERS AND AVERAGES OF POSITIVE AND NEGATIVE ERRORS FROM TABLES I–VI. SUBJECT D.

Series.	'A.'				'B.'			
	Positive.		Negative.		Positive.		Negative.	
	No.	Av.	No.	Av.	No.	Av.	No.	Av.
1	7	6.28	2	3.5	0	—	9	8.11
2	3	5.33	5	5.6	4	5.5	6	8.16
3	4	5.0	4	4.75	4	11.0	6	4.33
4	8	7.25	2	7.0	6	9.16	4	10.75
5	4	8.5	6	8.0	5	3.8	5	4.0
6	5	2.8	3	2.66	6	2.5	2	3.5
7	7	2.28	3	2.0	4	3.5	6	4.0
8	4	4.5	6	6.16	3	3.66	5	3.0
9	5	4.8	4	5.0	6	4.0	4	4.25
10	5	3.0	4	1.75	6	2.33	3	4.0
11	5	2.8	5	7.8	2	6.0	8	8.5
12	5	14.5	5	9.2	7	10.14	2	3.0
13	4	6.5	4	3.25	3	8.33	7	8.42
14	5	6.4	5	11.6	4	14.0	6	9.0
15	2	5.5	7	8.0	2	9.5	6	5.83
16	10	6.35	0	—	3	3.16	6	6.16
17	8	4.75	2	1.75	2	1.75	8	7.43
18	5	2.25	4	2.5	10	7.35	0	—
19	3	6.83	5	3.15	10	7.85	0	—
20	8	3.28	2	4.12	2	8.5	7	8.89
21	6	2.66	4	4.93	0	—	10	8.05
22	9	4.83	1	0.25	3	1.25	7	4.46
23	6	5.16	4	2.75	2	3.75	8	5.78
24	5	4.85	5	1.8	2	4.62	8	5.40
25	10	7.5	0	—	8	3.28	2	2.75
26	10	15.12	0	—	9	11.91	1	1.50
27	10	14.67	0	—	10	7.17	0	—
28	10	17.05	0	—	10	12.9	0	—
29	10	13.42	0	—	9	9.0	1	4.25
30	10	13.2	0	—	8	8.71	2	5.62

The variability of the numbers and magnitudes of the positive and negative errors within small limits, and the general tendency of the magnitude of the errors of the one kind to equal that of those of the other, with occasional large exceptions, is nicely shown. The first two series in Table X. and the last five of Table XI. are not included in this statement, because, as earlier explained, they belong under the natural fixation method.

The way in which the number of errors may work against the magnitude of error is illustrated by series 9 and 10 of Table X. The average positive error in 'A' of series 9 is only slightly greater than the average positive error in 'A' of series 10, and the average negative error is also somewhat larger in series 9. But the predominance of the positive errors in 9 and of negative errors in 10 makes the first average positive and the second negative (see Table VII.). On the other hand, there are relatively more positive errors in 'B' of series 5 than in 'B' of series 4; yet the greater value of the positive errors in series 4 gives the average of both positive and negative a more positive value in series 4 than in series 5. Similar effects of composition are to be observed in Table XI. These matters are not so important in work by the exact fixation method, as in the case of the natural fixation method. It is evident that a change in the average error in the latter case can be produced in either of two ways, and the change is not of much importance for explanatory purposes unless we know in which way it is produced.

Conclusion.

We may consider it as established that there are three general methods of judging the position of a moving pointer at the moment of a discrete stimulus, and that each of these methods produces characteristic results. The pointer-following method produces positive errors, the exact fixation method produces small errors, indifferent as to direction, and the natural fixation method produces errors depending on the retardation or anticipation in the reaction which is instrumental in the securing of an image of the pointer in a certain relative position.

It is safe to say that in the normal subject sensations whose peripheral processes are simultaneously excited are always perceived as simultaneous, unless the duration of one is much less than that of the other, and that sensations of different modality which are excited successively are apt to be perceived as simultaneous even when there is considerable interval between them. The errors in the judgments by the natural fixation method are in general much less than the time-threshold, and may even be zero, if the reaction happens to synchronize perfectly with the stimulus.

There is strong probability that the reaction in the natural fixation method is an eye-reaction. This is a matter for further investigation, and merges in the complex problem of vision in eye-movement.

The most immediate opening for work in connection with the remnants of the complication-problem is upon rhythmic reactions in general. The exact effects of rate, habituation, practice (*i. e.*, habituation with knowledge of results, and attempt to modify results in a certain way), type of stimulus, and type of reaction, in cases where the subject attempts to synchronize with or follow a rhythmic stimulus, must be ascertained.

THE PENDULAR WHIPLASH ILLUSION.[1]

BY ALGERNON S. FORD.

Professor Dodge, in an article entitled 'The Participation of the Eye Movements in the Visual Perception of Motion,'[2] attaches considerable importance to a certain phenomenon which he terms the 'whiplash illusion.' This illusion is obtained by fixating one of two moving lights which are attached to the two arms of a counter-balanced pendulum. The fixated light is perceived centrally, the non-fixated, peripherally. "When the point fixated approaches its extreme position in each oscillation, it seems to rest for an appreciable interval, while the other point seems to continue moving as though the two were connected by an elastic rod, which regularly gave the unfixated point a considerable additional oscillation after the fixated point had been arrested at the end of each swing. The illusion is persistent and striking, and is capable of only one explanation. It occurs at that part of the pursuit movement which photographic registration shows to be practically free from corrective movements. The fact that the point whose image remains motionless on the retina during an unbroken pursuit movement seems to stand still, while the other point, which is in reality moving no faster than its fixated companion, seems to make a little gratuitous whiplash excursion, serves at once to show the utter inability of the pursuit movement either to subserve the perception of motion of the fixated point or to correct the exaggerated data from the displacement of the retinal image of the non-fixated point."[3] Dodge has given a good description of the whiplash illusion, and he believes that this illusion sustains his theory that pursuit movements do not subserve the perception of motion. No positive theory, explanatory of the illusion, is found in the article in question, and it is by no means a certainty that

[1] From the Harvard Psychological Laboratory.

[2] Dodge, R., PSYCHOLOGICAL REVIEW, 1904, Vol. XI., pp. 1-14.

[3] *Op. cit.*, p. 14.

Dodge's claim, that the illusion is 'capable of only one explanation,' will hold after further experimental data have been submitted.

Dr. Harvey Carr, in an article[1] entitled 'The Pendular Whiplash Illusion of Motion,' takes a view opposed to that of Dodge, and suggests two possible explanations of this illusion. His first theory, which we shall for convenience term the *limen* theory, is given below: "As the pendulum approaches the end of its swing, the rate of movement gradually decreases to zero. Consequently, for some definite portion of the end of its swing, its rate would be below the eye movement limen, but still above the retinal limen of perceptibility. In other words, the retinally perceived light would be seen moving for an appreciable time after the fixated light had apparently stopped. Hence the gratuitous whiplash excursion is evident."[2] This positive assumption by Carr is supported by historical opinion. It has been commonly held that eye movements could mediate visual motion for the greater magnitudes and velocities, and that the limen of perception here is higher than the limen for retinal displacement.[3]

Carr gives most attention to his second or *positive after-image streak* theory. When the fixated light is followed properly, no positive after-image streak is appreciable, at least for the last portion of the swing. Unless the non-fixated light be very weak in intensity, it will leave an appreciable positive after-image streak. "The eye moves in a direction opposite to this latter light [non-fixated] and consequently the rapidity of its retinal displacement equals that of a light, perceived by a stationary eye, moving at a rate equal to the combined velocities of the two lights used in the pendulum test. Other things being equal, the length of the after-image streak varies directly with the rapidity of the retinal displacement. Thus a very pronounced length of the positive streak results in the test. This light, with its positive after-image, is viewed peripherally and hence is seen indistinctly and *en masse;* without conscious effort on the part of the observer, it appears as an elongated light with

[1] Carr, H., PSYCHOLOGICAL REVIEW, 1907, Vol. XIV., pp. 169–180.

[2] *Op. cit.*, p. 171.

[3] Aubert, H., 'Die Bewegungsempfindung,' *Pflüger's Archiv*, 1886, Bd. 39, S. 347–70; 1887, Bd. 40, S. 459–480.

no very decided contour, nor sharply discriminated parts."[1] As the pendulum approaches the end of its swing, this elongated mass of light rapidly contracts in length at its rear end because both the eye and the non-fixated light come to rest so that the after-image streak is no longer generated, and the older (here the rear) portion of the after-image fades the sooner. "If the positive streak is six inches long when the pendulum is one inch from the end of its swing, and this streak has time to disappear while the pendulum is moving and returning over this final inch of its arc, it is evident that the total mass of light will have contracted at its rear end from six inches to one inch in length. These values are of course merely illustrative. . . . Consequently, the whole mass of light will appear to be moving on, after the pendulum has really stopped. The observed extra movement is thus a purely illusory one."[2] It might be stated here that the author of the after-image theory does not assert this factor to be solely responsible for the illusion.[3]

The following conditions obtained in Carr's experiments. The lower arm of the pendulum was 78 cm. in length, the upper arm, slightly shorter. The lower arm was made to swing through an arc of 160 cm. Two seconds were consumed in a forward and return movement (complete swing). The observer was 230 cm. away from the lights. Two very small incandescent lights of low intensity were used. The tests were performed at night in a dark room.

The following conditions obtain in the present writer's experiments. Two miniature incandescent lights are attached to a counterbalanced pendulum. These lights are encased in wooden cups which have on their fronts circular openings of .5 inch diameter. The entire mechanism is placed behind a plate of ground-glass, the lights being brought very close to this glass. By careful regulation of the electric current and the use of small obstructive discs of milk-white celluloid, the intensity of the lights can be reduced to any degree. The lights are adjustable to any positions on the pendulum. Whenever it is desired to have a light at the axis, a third enclosed lamp is sus-

[1] Carr, *op. cit.*, pp. 171-172.
[2] *Ibid.*, p. 172.
[3] *Ibid.*, p. 178.

pended before the glass plate, and reduced to the required intensity. The best illusions are obtained by the writer when the lights are at short distances from the axis, *i. e.*, one inch to ten inches. The observer is stationed at a distance of seven feet from the lights. The tests are conducted in a dark room.

The pendular whiplash illusion is complex, its factors of illusion being: (1) the apparent unequal distances traversed by the two lights, (2) the apparent unequal speed of the two lights, and (3) the apparent movement of one light temporally longer than the opposite movement of the other.

I. *The Apparent Unequal Distances Traversed by the Two Lights.*—Dodge says: "If the distance of both from the axis was equal, both would move through equal distances in the same time. The one fixated however always appeared to move much less than the one seen peripherally. It was found that if the two were to appear to move through equal arcs, the pursued must actually move through about three times the arc of the unpursued. This of course could be accurately measured by the relative distances of the two points from the axis."[1] As regards his investigation of the above point, Carr has this to say: "My observers did not confirm these results as to the apparent lengths of movement. In fact, they gave judgments of equality of movement only when the two arcs were practically equal in length."[2]

My subjects gave judgments to the effect that the non-fixated light apparently moves a much greater distance than the one fixated. The difference of apparent distances traversed by these two lights is clearly shown in a modification of the experiment where a piece of cardboard, of sufficient size to allow the non-fixated light to be seen only at the very beginning and the end of its sweep, was placed over this light's path, this eliminating the after-image streak which might be thought responsible for the greater distance apparently moved through. The non-fixated light still appeared to travel much farther than the fixated. No exact measurements were made in the study of this factor (distance). It is undoubtedly significant that the non-fixated

[1] Dodge, R., *op. cit.*, pp. 13–14.

[2] Carr, *op. cit.*, p. 173.

light moves across the retina twice the angle that the eye-ball traverses in its socket in fixating the other moving light.

The illusion in the case of the modified experiment above was striking, though the positive after-image streak was eliminated as a factor in its determination.

For short arm lengths, there is a more marked difference between the apparent lengths of the arcs (described by the two lights) than for greater lengths, except where the opaque disc is used as already described.

II. *The Apparent Unequal Speed of the Two Lights.* — It seems natural that the non-fixated light should appear to move faster than the fixated, since it appears to move farther. It is the consensus of opinion of my subjects that such a difference is pronounced, yet no exact experimental data were secured to sustain such a view.

III. *The Apparent Movement of One Light Temporally Longer than the Opposite Movement of the Other.* — This factor is the whiplash illusion proper. The references and theories cited in the first part of this article, bear directly upon this factor.

There is an appreciable difference in the quality of the illusion for great, and very short lengths (of the pendulum arms). In case of a ten-inch length of the arms, the pendulum appears to bend at or near the axis, while in one- or two-inch lengths the pendulum appears rigid, the non-fixated light depending from the fixated as a point of support.

When very short lengths are used, so that the lights move slowly, a few of my subjects observed practically no motion of the fixated light.

It makes no difference in the illusion itself whether the lower or upper light be the one fixated. The illusion is equally appreciable for horizontal, vertical, and oblique oscillations. In the latter two cases however, greater difficulty of fixation is experienced. This is probably due to the comparative difficulty of vertical and oblique eye movements.

An interesting variation of the experiment is made by placing one of the two lights at the axis. When the moving light is fixated, an illusory motion of the stationary light is observed.

In order to establish a pendulum arm-length (within reasonable limits) at which the illusion is most pronounced, the contents of Table I. (which follows) were obtained. The several degrees of illusion are reported as Weak (W), Medium (M), and Strong (S). The absence of illusion is expressed by the letter N. Intermediate degrees are indicated by the plus and minus signs. For the purpose of numerical comparison, N, W–, W, W+, M –, M, M +, S –, S, S +, and S ‡, are represented respectively by the arbitrary values 0, 1, 2, 3, 4, 5, 6, 7, 8, 9 and 10.

TABLE I.

UPPER LIGHT FIXATED AND ATTENDED TO.

Name.	10″–10″			8″–8″			6″–6″			4″–4″			2″–2″			
H	S	S	S	S–	S–	S	S	M	S	M	M	M	S	S	S	→
R	S–	S–	S–	M+	S–	S–	S	S	S	S	S	S–	S+	S+	S+	→
F	S	S–	S	M–	M	M+	S–	S–	S	M+	S–	S–	S–	S	S+	←
R′	W	M	M	M	S	M	S	S	S	S+	S+	S+	S‡	S‡	S‡	→
N	M+	M	M+	W–	M+	S	M+	S	M+	S	S	S	S+	S+	S+	→
M	M	M	M	M	M	M	S	S	S	M	M	M+	S	S+	S+	←
M′	S+	S+	S+	S–	S–	S–	M	S–	S–	N	M–	M–	N	W	N	←
N′	W	M	S	M	M	S	W	S	S+	M+	M	S–	M	M	M	←
Num. Equiv.	154			144			173			151			175			
Av. Equiv.	6.4+			6.0			7.2+			6.3+			7.3+			

NOTE:—(1) Figures at the top show the distances of the two lights from the axis of the pendulum. (2) Arrows indicate direction of procedure with the several subjects. (3) Each subject gave three judgments for each position of the lights. (4) Oscillations are horizontal; upper light fixated. These conditions prevail throughout the experimentation except for Table V when a third, or central, light is fixated.

From the, to be sure, somewhat irregular results of Table I., it would seem that the shorter lengths of the pendulum arms favor the illusion.

A modification of the regular experiment, made by placing a grating over the fixated light, gave very interesting results. In case the grating was placed over the fixated light while the pendulum was in motion and the illusion appreciable, a diminution of the illusion followed instantly. The grating adds the factor of image displacement on the retina to the usual perception of the motion (whatever this consists in) of the fixated light. This gives evidence in support of Carr's *limen* theory.

Table II.

Grating Used Over Fixated Light.

Name.	10^{-8}"			10^{-7}"			10^{-6}"			10^{-5}"			10^{-4}"			10^{-3}"			10^{-2}"			10^{-1}"		
H	W	W	W	W—	W—	W—	W	W	M	W—	M	W	N	N	N	N	W—	W—	N	N	S—	N	N	↓W—
R	S—	M+	S	M+	M+	M+	W+	W+	M+	W	M+	S—	W+	M—	M+	W	M—	M—	M—	M—	S—	M+	M+	↓S
F	S—	S—	S	M+	S—	S—	S	M+	M+	M	M	M	M	S	M	W	S	M	W	W	W	W—	W—	↓N
N	M	M+	M+	W	W	M+	W	W	M—	W	W	M	M	M—	S—	S—	W	M—	W	M	M	M	S	↓M+
No. Eq.	69			51			49			47			47			40			40			41		
Av. Eq.	5.75			4.25			4.08+			3.91+			3.91+			3.33+			3.33+			3.41+		

Note: (1). Subjects *R′*, *M*, *M′* and *N′* were not available for this experiment.

By a comparison of Tables I. (or III.) and II. it is seen that the grating diminishes the illusion considerably. Out of ninety-six judgments in Table II. there are ten where the illusion was absent altogether.

Carr states (p. 177) that the illusion 'is conditioned by the direction of the attention.' In obtaining Table I. the importance of this statement was frequently realized. In preliminary tests seven out of eight subjects reported the illusion to be better when the attention was directed to the fixated light than when directed to the peripherally perceived light. It seems that when the fixated light is attended the non-fixated light is neglected, and so is permitted to execute its movement without in any way being checked, in other words, it is allowed to go free, whereas, in the event that the non-fixated light is attended, it is kept vividly in consciousness, and therefore we seem to *keep pace* with it and to perceive its motion as being perfectly normal. This led to the formation of Tables III. and IV., the results of the former being obtained by carefully attending the fixated light, those of the latter, by attending the non-fixated light.

The values of *R*'s judgments in Tables I. and III. are respectively, 143 and 132 points. This shows a situation contrary to his introspection that it is when the non-fixated and not the fixated light is attended that the illusion is greater. With the exception of *R*, it was reported by the observers in the experiments of Table IV. that whenever the illusion was perceived, it was distinctly felt at the time that the attention had wandered *back* from the non-fixated to the fixated light or to some intermediate point.

The decrease in the degree of the illusion (in Table III.), as indicated by the numerical equivalents, is very likely due to the distracting influence of the non-fixated light upon the attention as the lights become closer together. In other words, it appears that the attention, at short distances of the lights, is inclined to pass to the non-fixated light, thereby diminishing the value of the illusion.

We observe in Table IV. a general decrease toward the smaller distances. This appears reasonable, *for the non-fixated light can be more easily attended at the shorter distances.*

Table III.

Attention on the Fixated Light.

Name.	10″—8″			10″—7″			10″—6″			10″—5″			10″—4″			10″—3″			10″—2″			10″—1″		
H	M	M	M	S—	M+	M	M	M	M	M+	M+	M	M+	S—	M	M+	W	M	S—	M	M+	W	W	→W
R	S—	S—	S—	S—	S	S—	M+	S—	M+	M—	M—	M+	S	S—	S—	M+	M+	M	M—	M	M+	M—	M—	→M
F	S‡	S‡	S+	S+	S+	S	S+	S+	S+	S+	S	S+	M	S	M+	S—	S—	M+	M+	M+	M+	W+	M—	←M
N	S	S	S	S	M+	S	M+	S	S	M	M+	S	M+	M	M+	M	M+	S	M	M+	M+	W	W	←M
N′	M	M	M	M	M+	M+	S—	M	S	W	M	W	M	M+	S+	S+	S+	S+	S	W	M	W	M	→M+
No. Eq.	104			105			103			85			96			96			82			53		
Av. Eq.	6.9+			7.0			6.9+			5.7+			6.4			6.4			5.4+			3.5+		

TABLE IV.

ATTENTION ON THE NON-FIXATED LIGHT.

Name.	10″–8″			10″–7″			10″–6″			10″–5″			10″–4″			10″–3″			10″–2″			10″–1″		
H	W–	N	N	N	N	N	N	W–	N	N	N	N	N	N	N	N	N	N	N	N	W–	N	N	←N
R	S–	S–	M+	M+	M+	S–	M	M–	S–	M	M	M	M+	M+	S–	M	M+	W+	M	M	M	M	M	←M–
F	W	N	W+	M	W	N	W	M+	M+	W–	W–	N	W–	N	W–	W–	W–	W–	N	N	M	N	M	→N
N	W	N	W+	N	W	N	N	W	N	W	W	N	N	N	W	W	N	N	N	N	W	N	N	←N
M′	W	W	W	W	W	N	N	N	N	W	N	W	N	N	W	W	N	W	W	N	N	N	N	←W–
No. Eq.	37			32			33			20			25			23			25			20		
Av. Eq.	2.5+			2.1+			2.2			1.3+			1.7+			1.5+			1.7+			1.3		

Note: (1) The light *ten* inches distant from the axis is the one fixated during experimentation for Tables III. and IV. (2) *R′* *M*, and *M′* were not available as subjects for Table III.; *R′*, *M*, and *N′* not available for Table IV.

In order further to test the influence of direction of the attention on the illusion, the following variation of the experiment was made. Three lights were used, two as before and the third made stationary at the axis of the pendulum. At all times this *central* light was to be fixated. After-image streaks were eliminated by reducing the intensity of the lights. The observer was given a position seven feet from the lights. His head was held stationary by clamps. A small light was thrown from the side on one eye, the other eye being covered.[1] A reading telescope was trained upon the eye so that the slightest motion could be detected. The two peripheral lights were each eight inches from the axis. The observer was instructed to direct his attention to *one* of the peripheral lights, his fixation being on the central light however.[2] Under these conditions the non-attended light regularly appears to move for a longer time than the attended light. The observer then reported each time that the illusion appeared. Fifteen judgments of illusion were taken for fixation of both lower and upper lights. The illusion is readily perceptible, but appears irregularly, depending upon the accuracy of the attention's direction. By the use

Table V.

Three Lights: Central Being Stationary and Fixated.

Name.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
H	o	o	o	o	o	o	o	o	o	o	m	o	o	o	o
	o	o	o	o	o	o	o	o	o	o	o	o	o	o	o
R	o	o	o	o	o	o	o	o	o	o	o	m	o	o	o
	o	o	o	o	m	o	o	o	o	o	o	o	o	o	o
F	o	o	o	m	o	o	o	o	o	o	o	o	o	o	o
	o	o	m	o	o	o	o	o	o	o	o	m	o	m	o
N	o	o	o	m	o	o	m	o	o	o	o	o	o	o	m
	o	m	o	o	o	o	o	o	m	o	o	o	o	o	o

Note: (1) o = no eye-movement; m = eye-movement.

(2) For every judgment (whether o or m) there was an illusion.

(3) Each of the two pendulum lights 8 inches from the axis.

(4) Upper row of judgments for each subject is given for case where the upper light is attended, the illusion being of the lower light; the lower row of judgments, when the lower light is attended, the illusion being of the upper light.

(5) *R'*, *M*, *M'* and *N'* were not available as subjects for this experiment.

[1] In all previous experiments, both eyes were used.

[2] As to the possibility of such an adjustment, cf. Helmholtz's *Physiol. Optik*, 2te Auflage, 1896, S. 605.

of the telescope, the experimenter was able to note any eye-movement at or immediately preceding the instant of report. Out of one hundred and twenty judgments as seen in Table V., there were only twelve where the experimenter was at all uncertain as to the immobility of the eye.

This last experiment shows conclusively that the illusion is to a large degree influenced by the direction of the attention. The effect produced by the attended light is perceived *prior* to that of the non-attended light. When the attended light has reached the end of its swing, the non-attended light is of course neglected and slow to enter consciousness. When the non-attended light appears to be reaching the end of its swing, the attended is on its way back, having been transmitted to consciousness before the former. The apparent bending of the pendulum occurs at this time. *It is evident that the pendular whiplash illusion is in part a special case of the law of prior entry of attention.*[1]

Of the three factors of illusion, that of the whiplash *excursion* has received most attention at the hands of the investigators, but this in no wise should indicate that the factors of apparent distance and velocity of the lights are unimportant. Dodge has failed to give a theory accounting for the illusion. He uses it (the illusion) as an instrument in his attempt to prove that the pursuit movement is unable 'to subserve the perception of motion.' Carr offers the *limen* and *after-image* theories both separately and conjointly in explanation of the illusion. The *limen* theory, in addition to claims already advanced for it, is supported by Table II. of this article. There is no incompatibility between the *limen* theory and the *attention* factor. The positive after-image streak is possibly a factor of some importance, but it is well to remember that the illusion can be produced without the influence of this streak, as in the case where an opaque disc was placed over the path of the non-fixated light. The *after-image* theory is further weakened by the fact that when the after-image was eliminated (as above when a piece of cardboard was placed over the non-fixated light's path) the

[1] See Titchener, *The Psychology of Feeling and Attention*, p. 251, for the law of prior entry.

distance (apparent) traversed by the non-fixated light was *greater* than before. The results of Tables III., IV. and V. sustain the factor of *attention*.

From the foregoing investigation we seem bound to conclude that the pendular whiplash illusion is a complex phenomenon depending on one or all, according to the conditions, of the following factors: (1) the fading of after-image streaks at their older ends; (2) the lower threshold of movement perception by means of displacement of the retinal image, as compared with the threshold of movements as perceived by movements of the eyes; (3) the law of prior entry of attention.

JUDGMENTS ON THE SEX OF HANDWRITING.

BY JUNE E. DOWNEY,
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In his book entitled *Les révélations de l'écriture d'après un contrôle scientifique*, Binet cites an investigation undertaken to decide whether it is possible to determine sex from handwriting. His conclusion is that within certain limits it is possible to do so. The question whether the sex-difference so discovered be due to psycho-physiological or social causes is left open.

It seemed to me worth while to repeat Binet's test in this country where, at least, the variation in writing induced by sex segregation in education would be minimized. A further motive for the test was furnished by the fact that the passing of such a simple judgment as 'man's writing' or 'woman's writing' seemed to offer particularly good material for a study of certain phases of judgment.

In the present test Binet's method was followed closely except in the particulars cited below. Briefly, his procedure was as follows. One hundred and eighty envelopes that had for the most part passed through the mails but from which all seals, headings and the like had been removed were submitted to two professional graphologists and to fifteen persons ignorant of the art of graphology. After a careful study of each superscription, the observer recorded his judgment as to the sex of the writer. Of the envelopes, eighty-nine had been addressed, either to Binet himself or to members of his family, by women; ninety-one had been addressed by men. As no selection had been made of envelopes to be used in the test the collection was held to be a representative one. Binet cites as a source of error the tendency to infer the sex of the writer from the sex of the person addressed.

This source of error was avoided in the investigation to be reported since all envelopes used were addressed to a woman. Two hundred envelopes were employed; all but four had

passed through the mails. One hundred had been addressed by women; one hundred by men. A considerable number of the two hundred persons whose writing appears in the series are known to have been educated wholly in co-educational schools. One hundred and fifty of the envelopes were collected by Dr. Grace R. Hebard as secretary of the board of trustees at the University of Wyoming. These envelopes were all superscribed to Dr. Hebard at the same address. They were numbered by her in the order in which they were received so as to insure a chance grouping within the series. The other fifty envelopes were addressed to me or to my sisters. The writers represented in the series include a considerable number of grade teachers and of college and university professors, many business men and women, a few lawyers, doctors and ministers, a few society women and women of leisure, a few literary people. On the whole, the collection is representative, although the teaching profession is unduly conspicuous.

These two hundred envelopes were submitted to thirteen persons, each of whom recorded his judgment as to the sex of the writer who had addressed each envelope. Whenever the writing on an envelope was recognized the judgment was thrown out. For the most part, however, the possibility of such recognition was slight since a very great number of the envelopes had been addressed by strangers to the community. None of the thirteen persons made any pretense to skill in reading handwriting; none was acquainted with the particular claims of graphology. Some of them had had, however, much more extensive opportunity to observe varied forms of penmanship than had others. In age, the observers varied from fifteen years to something over fifty. Eight of the observers had had some slight training in psychological experimentation; the others, none at all. Beside these judgments on the complete series of two hundred, five other persons gave judgments on a series of one hundred envelopes.

If in such a series of judgments, where there are only two possibilities to choose from, the number of right judgments exceeds fifty per hundred, it is evident that something other

than chance dictates the judgment. In every case in the present test, as in the French test likewise, the percentage made was sixty or over. The results, in fact, parallel closely those obtained by Binet. The percentages of right judgments given by ten of his observers (neither of the experts is included) ran as follows: 65.9, 66.4, 67, 68, 69, 69.3, 72.9, 73, 73, 73.[1] The percentages made in the present test were as follows: 60 (*P*), 60 (*C*), 61.5 (*B*), 64 (*Py*), 66 (*Ck*), 66.5 (*A*); 68 (*Bh*), 68.5 (*R*), 70 (*D*), 70.5 (*S*), 71.4 (*Sn*), 71.5 (*Cn*), 77.5 (*Ra*).

Binet concludes that it is possible, with a certain percentage of error, to determine sex from handwriting. Obviously in estimating this error it is permissible to take the best record made by any observer. In the French test this record was made by the professional graphologist, M. Crépieux-Jamin, with 78.8 per cent. of correct judgments.[2] Furthermore, it is of course possible, though hardly proper under the circumstances, to argue that greater natural facility and a better grounded science of graphology would reduce the percentage of error. For instance, *Ra*, who made the record in my test, was allowed to review her errors in the first hundred judgments and then asked to repeat the test for the second hundred. Her percentage of correct judgments rose to 80. In any case, however, the possibility of a considerable percentage of error makes the determination of sex from handwriting of problematic value where a high degree of certainty is required as in legal procedure.

It would be possible in certain ways to strengthen Binet's conservative conclusion as to the validity of judgments passed on so-called sex-differences in handwriting. In the present investigation, for example, I asked the observers to enter their judgments under the following rubrics: Confidence Great, Confidence Moderate, Confidence Slight, and Confidence None. If now the percentage of correct judgments be calculated from the number entered under the head of Confidence Great, we get the following figures in place of those given above: 66.3 (*B*), 67.4 (*C*), 70.5 (*D*), 70.9 (*Bh*), 72 (*A*), 72.1 (*R*), 72.7 (*Ck*), 80.1 (*Cn*),

[1] *Les révélations de l'écriture*, p. 11.

[2] *Op. cit.*, p. 8. I am unable to get this percentage from Binet's figures. According to my reckoning the percentage should read 78.3.

81.1 (*Ra*), 82.4 (*Sn*), 82.9 (*Py*), 83.3 (*S*). The percentage of *P* is not included since he failed to use the exact rubrics named in the instructions. In every case given it is evident that the percentage of error has been considerably reduced. Within a certain range the revelation of sex appears to be quite evident. Binet shows also that M. Crépieux-Jamin's percentage of correct judgments rose when this percentage is reckoned on the basis of the judgments that he considered certain in distinction from those that he considered only probable.[1] Under conditions most favorable to the expert, according to Binet's report, the error is reduced to ten per hundred.

Another fact emphasizing the validity of the judgments under investigation is brought out by a further study of the results of the present test. The judgments of the thirteen observers on the two hundred envelopes were tabulated in such a way as to show the concordance of judgments on the part of the observers. The judgments of twenty-one of the two hundred envelopes showed complete unanimity; in the case of forty-two other envelopes there was only one dissenting judgment, so that in sixty-three cases there was practical unanimity of judgment on the part of the observers. Further, in all but fifty cases there was a strong preponderance of judgments (four or more) in favor of one or the other of the sexes. Such facts show conclusively that there exists, for the average person, a fairly definite conception of masculine and feminine penmanship.

The bearing of the evidence upon the fact of actual sex-differences is, however, blurred by what Binet calls the inversion of sex signs. In my test, of the sixty-three envelopes on which judgment was practically at one, nine were written by the sex opposite to that to which they were ascribed. In the 150 cases showing a clear preponderance of judgments in one direction or the other, twenty-seven such inversions occur. Binet gives reproductions of writing showing inversion of sex signs but fails to state the number of times such inversions occur — an unfor-

[1] *Op. cit.*, p. 9. I cannot determine from Binet's figures the percentage of correct judgments calculated from the number given with certainty. So far as I understand the figures the percentage is 82.3, closely approximating the best record made in my test.

tunate omission which renders impossible a detailed comparison of his results with mine.

Again, examination of the present results shows something of which Binet makes no mention, namely, that these so-called inversions of sex signs occurred somewhat more frequently in the case of women than in the case of men; that is, the twenty-seven cases of notable inversion include seventeen cases of women's writing uniformly ascribed to men and only ten cases in which the reverse occurred. Furthermore, the records show a distinct preponderance of masculine judgments. Of the 2,592 judgments passed 1,351 were masculine, 1,241 feminine. The reason for this excess of masculine judgments is evident from the statements of certain observers relative to what they considered to be the signs of masculinity and femininity in handwriting. Obviously, originality is held to characterize the man's hand; conventionality, the woman's. Consequently, masculine handwriting is thought to show a more extensive range of variability than does the woman's. The plain evidence of such a constant drift in judgment is interesting in view of the fact that it follows the conclusion of certain scientists that men are much less conventional than women and show a greater range of variability. In the present case such a pressure of social judgment (if one may so express it) led to a constant error.

In this connection it is profitable to study the handwriting uniformly judged masculine or feminine. The typical feminine hand does appear to be colorless, conventional, neat and, usually, small. I should judge it unlike the feminine hand of the French test. In four cases alone does it exhibit individuality. Two of these four exceptions were graceful flowing hands; one an excessively rounded hand; one a backhand. Frequently this hand shows signs of unaccustomedness, that is, of being the handwriting of a person who had done no great amount of handwriting either because of youth or manner of life. Such unaccustomedness leads to conventionality in writing. This feminine hand is also in many of the cases the handwriting of grade or high-school teachers, whose profession emphasizes conventionality of writing. The typical man's hand is bold or

careless or experienced, above all, individual. It is written by professional men and women or by newspaper and literary folk.

Let us now study the twenty-seven cases showing inversion of sex characteristics. The ten masculine writers of feminine handwriting were as follows: one farm hand, one college student, two school teachers, two college professors, one business man, one lawyer, one minister, one left-handed superintendent of public instruction. The first four wrote the colorless conventional, somewhat unaccustomed hand already described; the second five wrote small neat flowing hands; the tenth penman wrote a labored backhand. At least two of these men have done no great amount of writing; four others are teachers.

The seventeen women whose writing was judged masculine were as follows: three county superintendents of public instruction, one public lecturer, one university secretary, three women who have held clerical positions, one college professor, four school teachers, two society women, two old ladies of over seventy years, one of whom had served for years as a librarian. Of these women, seven wrote either very bold or very rapid hands; six wrote very individual hands; the other four wrote careless sprawling hands. The point to be noticed is that at least half of the number have been accustomed to more than a usual amount of handwriting. This fact would apparently lead to a social rather than a psycho-physiological interpretation of sex-differences in handwriting. A reversed interpretation is of course possible since it is open to argue that original mental differences are basal and that the inversion of sex signs in handwriting points to an inversion in other respects. The trend of my results makes me exceedingly doubtful of such an interpretation. Granting the existence of sex mental differences based on profound psycho-physiological causes, one would scarcely expect to find an inversion of such differences eighteen times per hundred cases.

In this connection it is well to note the hand called by Binet ambiguous. It gives little evidence of sex. In the present test there were found twenty-one cases in which the judgments were practically evenly distributed between the two sexes. A study of the envelopes so grouped reveals in general two sorts of

handwriting. The one is, again, the manifestly inexperienced hand; the other a small supple hand, less bold than the so-called masculine hand, more individual than the so-called feminine hand. This latter hand appears to be, frequently, the handwriting of highly cultured persons.

Attention should also be called to age as influencing handwriting. Naturally the unaccustomed, hence conventional and labored, hand would more frequently characterize the younger penmen, solely on the ground of lack of experience. The writers producing in the present test the typical woman's hand probably averaged less in age than did those writing the man's hand, although there were striking exceptions. Crépieux-Jamin would ask that the handwriting of very old people be eliminated in a test on the revelation of sex in handwriting. It was evident that the very old ladies whose writing appeared in my series did not produce the typical woman's hand.

To sum up. From my analysis of my own results I conclude that it is possible to determine sex from handwriting in perhaps eighty cases out of a hundred. A detailed study of the cases showing inversions of judgments leads me to believe that the presence or absence of the so-called sex-signs is, in the case of any one writer, influenced largely (1) by the amount of writing done; (2) by age and consequently, to a certain extent, by practice; (3) by professional requirements such as shown by the conventional writing of grade teachers and the rapid hand of bookkeepers. As the majority of grade teachers are women and as women other than teachers as a general rule do less writing than men (professional men at least) women's writing on the average would be distinguishable from that of men.

On the face of it my results as presented are closely comparable to those obtained by Binet. Whether they are actually so could only be determined by a comparison of specimen with specimen. Particularly one would wish to compare the typical feminine and masculine hands of the two tests and above all the ambiguous hands and those showing inversion of sex signs. Such a detailed comparison would show how far the judgments passed are themselves of social origin.

My interpretation of my results would not, of course, be in

line with the claims of professional graphologists who inspect handwriting for the discovery of such signs of femininity as lack of energy, of clearness and of simplicity, and exhibition of vanity and self-consciousness.[1] On more general grounds than this one might expect a difference in the handwriting of the sexes, since previous investigations have given us reason for believing that men exceed women in motor ability.[2] The application of such a general principle to sex-differences in handwriting must, however, be turned over to those psychologists who are experimentally determining the range of variation in writing reactions.

In conclusion, a word relative to the types of judgment revealed by the experiment. The varying skill shown by the subjects of the test was most interesting. The proficiency displayed by one or two of them actually gives some color to the claim of those who believe in the absoluteness of the sex-revelation in handwriting and charge error to lack of insight. On the other hand, the fact that amateurs are able to approximate the record made by professionals who have given much time and thought to the art may be cited as showing that the errors of the latter may be charged not to lack of proficiency in interpretation but to the fact that there is an easily attained limit to the power of such interpretation. Not all handwriting does reveal sex.

Binet observed that, on the whole, the ability of his subjects to give correct judgments far outran their ability to ground those judgments in definitely assigned reasons. Certain of his subjects were much more deliberate than were others who satisfied themselves with a glance of the eye. Crépieux-Jamin, on the other hand, furnished Binet with an elaborate analysis of the signs of sex discerned by him in the case of each specimen passed upon. Binet was thus furnished with a mass of material to be correlated with correct and false judgments respectively. In this respect I was unable to duplicate his test. I made, however, some attempt to determine certain facts in regard to the judgments passed.

[1] J. Crépieux-Jamin, *L'écriture et le caractère*, p. 385 f.

[2] H. B. Thompson, *Psychological Norms in Men and Women*, Chap. II., p. 8 f.

It was desired to determine, first, whether the more deliberate or the more intuitive observer would give the higher percentage of correct judgments; secondly, to note individual differences in the matter of degree of confidence, and to determine the index of accuracy with assurance.

The judgments were entered under two all-inclusive headings. Judgments Immediate and Judgments Hesitant, the observers being instructed to enter a judgment under the second head whenever there was any pause for deliberation. Under each of these headings were placed the rubrics, Confidence Great, Confidence Moderate, Confidence Slight and Confidence None. A record was kept of the time taken by each observer in passing the judgments, this record to serve as an objective check on the division made by the observer into immediate and hesitant judgments. This check was somewhat crude, inasmuch as the time might be lengthened through mechanical slowness in the recording of judgments which had been quickly passed. In general, however, the subjects giving the greater number of immediate judgments performed the test in a shorter time than the other observers. *Sn*, who gave the greatest number of immediate judgments (89.7 per cent.), also carried out the experiment in the record time, sixty minutes. *A*, who gave the fewest immediate judgments (55.5 per cent.) required, with one exception, most time for the experiment, 115 minutes.

The immediate judgment represented a judgment on the writing as a whole, with little attention to details. The judgment is simply an identification of the writing as man's or woman's or a mediate classification on the basis of some rubric, as 'bold' hence 'masculine'.

Reference to the percentage of right judgments shows that *Sn* and *Ra*, who gave the highest percentages of immediate judgments, made respectively third and first place in accuracy of judgment on the whole series. *Ck*, *Bh*, *B* and *Py*, who ranked third, fourth, fifth and sixth with reference to immediacy of judgment, relative to accuracy, took, respectively, the ninth, seventh, eleventh and tenth positions. On the whole, therefore, a certain amount of hesitation appears to be of advantage.

Estimating the percentages of correct judgments for immediate and hesitant judgments, it is found, however, that *with one exception*, the immediate judgments give uniformly a higher percentage of correct judgments than do the hesitant. Partly, no doubt, this is due to the fact that it is the more difficult cases that induce hesitation. The subjects, however, shift their relative positions when ranked by reference to the accuracy of their immediate or hesitant judgments. Hesitation is, for certain observers, fatal, they are hopelessly confused by it. *D* improves in her judgment; *Ck*, who in other matters is quite capable of analysis and assisted thereby, appears in this test temperamentally adverse to hesitation. The hesitating judgments of *S* are very nearly as accurate as his immediate ones. Whether, then, it is better in this test to depend upon intuition or deliberation appears to be an individual matter.

Turning now to the certainty or confidence with which judgments were given one finds again a great range of variation. The percentages of judgments given with great confidence ran from 23.5 to 82.5 per cent., the no-confidence judgments from 0 to 21.8 per cent. Three of the observers recorded only three degrees of confidence; one confined herself to two only, namely, confidence great and confidence slight. A comparison of the percentages of correct judgments on the whole series with the percentage correct when confidence was great shows, as previously mentioned, a higher degree of accuracy when confidence is great. The individual differences are, again, notable. *D* gives a rise in accuracy of only .5 per cent.; *Py* gives an increase of 18.9 per cent.

A ranking of observers on the basis of their accuracy with assurance would be of prime importance if one wished to use such judgments in an objective way. Such a ranking would effect considerable shifting of relative position. *S*, who held fourth place in general accuracy with 70.5 per cent. of correct judgments, now moves to first place with a percentage of 83.3 correct judgments, an approximation of the professional record. *Py*, who held tenth place with 64 per cent. of correct judgments, now takes second place with 82.9 per cent. of correct judgments. Both *S* and *Py* are, however, exceedingly cautious,

giving, respectively, only 24.3 and 23.5 per cent. of confidence-great judgments. *Ra* and *Sn* still maintain their position among the first four most accurate observers, *Ra* with a percentage of 80.1 correct judgments on 58 per cent. of confidence-great judgments, *Sn* with 82.4 per cent. of correct judgments on 58.1 per cent. of confidence-great judgments. *Ra* and *Sn* are thus seen to be altogether the most valuable subjects since their index of accuracy is high and their assurance covers a considerable range.

There was some tendency for a high percentage of confidence-great judgments to be correlated with a high percentage of immediate judgments, although the range in the latter case is less than in that of the former. In every case there are but few hesitant judgments entered under the head of confidence-great; at most, only seven per cent. of the total number in the series.

On the whole, the individual differences thrown into relief by the test were interesting and striking. The rapidity with which some observers reached their decisions was in great contrast to the slowness of others. The variation in the assurance with which certain observers recorded their judgments contrasted with the lack of confidence exhibited by others. Noticeable, too, was the great variation in the value of the confident judgments of the different observers. One is not ready to conclude that the results of the present test would be typical of the same individual when dealing with other material, although the fact that no observer had had previous experience in passing such judgments put them, in this respect, all on the same level in the test under consideration. For all of them the test was a novel one. It may be said that the young woman giving the greatest number of great-confidence judgments (82.5 per cent.) bears a reputation in the college community of being confident and decided in her opinions; the one giving the lowest percentage of confidence-great judgments (23.5 per cent.) is peculiarly self-distrustful and diffident.

Two methods of basing the judgment were described. The first method was analogical, based on vague popular inductions concerning masculine and feminine characteristics. The second

method involved a reference to types or classes of handwriting, a more or less clear-cut classification dependent upon one's experience. The more extensive this experience and the more interest had been directed to the inspection of handwriting, the more this method prevailed. A distinct reference to one or more similar hands was often remarked. For some, this reference seemed to be to a generic rather than to a specific image. One subject who carefully reviewed her judgments with the envelopes and the list of penmen before her stated that a frequent error had been the reference of a whole class to the wrong head. Her classificatory type was clear-cut but its label 'masculine' was incorrect. How far these methods of judgment could be described as contributory, respectively, to logical or psychological certainty is not evident. Judgments given with confidence occurred in both cases. My records are not complete enough to show whether individual variations occurred in the degree of confidence accompanying these two sorts of judgment.

THE CHANGE OF HEART RATE WITH ATTENTION.[1]

BY M. LEROY BILLINGS, A.B., AND JOHN F. SHEPARD, PH.D.,

Writers on the relation of mental and nervous conditions and organic changes, such as those in the circulation, have usually treated the various organic reactions as though they were relatively independent of each other. Studies of the influence of stimuli upon the heart rate have been made without a sufficient consideration of the breathing and Traube-Hering waves, and especially of the effect of change in the depth and rate of breathing. In an earlier work[2] by one of the writers it is suggested that the temporary slowing of the pulse which one often gets with sensory attention is due to the decreased rate and amplitude of the breath under these conditions. While the results discussed there suggested such an explanation, the point needed further study. This paper is a report of the results obtained from experiments designed to show more definitely the factors concerned.

To get the pulse in these experiments we used the Hallion-Compte plethysmograph. It was connected with a piston recorder writing upon a kymograph. For the breath we used Sumner's pneumographs, one around the chest and one around the abdomen. These were connected to Marey tambours. The kymograph used was the Zimmermann with a long extension paper about eight feet in length. Writing upon this same kymograph, in a vertical line with the piston needle and the tambours, were an electric marker to indicate when the stimulus was given (in referring to this line hereafter we shall call it the reference line), and another which marked the time in fiftieths of a second. This latter was run by an electric current interrupted by a tuning fork. In these experiments the drum turned rapidly

[1] From the Psychological Laboratory of the University of Michigan.

[2] Shepard, 'Organic Changes and Feeling,' *The American Journal of Psychology*, 1906, p. 554.

enough so that the time could be counted very accurately in two-hundred-fiftieths of a second. The recording apparatus just described was placed in one room and the subject in another so that the noise of the operator and the running of the kymograph did not disturb him.

The subjects for these experiments besides one of the writers, Mr. Billings, were Mr. Dockeray, a graduate student and assistant in psychology, and Mr. Carpenter, an undergraduate student who was also taking special work in psychology. We wish to acknowledge our indebtedness for their kind assistance.

A number of records were taken simply to show the effect of retarded breathing. A normal curve was taken for some seconds, and then the subject was given a signal to consciously restrict his breathing. Likewise the effect of increased breathing was studied. Then different types of attention were investigated; first, attention to some central process such as recalling the forms of a French verb; secondly, attention to a visual stimulus such as reading at a distance; and thirdly, attention to an auditory stimulus, trying to detect a faint sound. Immediately after a record for a sensory attention was taken, another was taken in which the subject was asked to reproduce the changes of breathing of the attention record as nearly as possible. In the first tests he was merely asked to begin at a signal and to repeat the method of breathing of the attention experiment as nearly as he could from memory; but, in the latter experiments we set up another kymograph (number two) directly in front of the subject with a pneumograph and tambour as before. Then, while the subject was working, we were getting a curve of his respiration on this kymograph as well as on the main one. Now, when he was to reproduce a record we raised the kymograph (number two) so that the point of the recorder came just under the original tracing. By this method, when the apparatus was going, the subject could reproduce exactly the original record. In the interpretation of these latter reproductions, we must remember that we have a sensory stimulus in each one of them as well as in the original record. So the only difference between the two is in the normal. In the original record we have a normal curve followed by a curve influenced

by a sensory stimulus, while in the reproduction we have at the beginning a curve made by a normal breathing plus a sensory stimulus followed by an identical reproduction of the original.

After taking thirty-seven records we worked them up carefully as follows: The beginning of every pulse beat was marked with a sharp needle under a magnifying glass, and from each one of these points was erected a perpendicular to the time line. Then, the length of each pulse was counted off between these perpendiculars to two-hundred-fiftieths of a second. The errors in this counting are very slight if any. And in any case an error of one or two units would be of no importance.

For plotting the breathing curve we drew a line parallel to the reference line which represents the position that the tambour needle would have held had it not been disturbed by the movements of the breathing — in other words, the level line of the tambour-needle. This was used as a new base-line. From this the height of the breath at the beginning of each pulse and at the crest and trough of each breath was measured in millimeters and plotted.

The plotted records do not always show directly how accurately the breathing was reproduced, for since the plotted rate is measured in pulse-beats, and these are faster in one case than in the other for different reasons, the apparent length of the breath varies more than the original curve would show.

After having worked up all these records the results were plotted on coördinate paper. The X axis, or the standard line for the heart rate, was given the value of thirty-five, forty, or forty-five fiftieths of a second (represented as P_{35}, P_{40} or P_{45} at the left end of the curve), and each unit of space above or below was given the value of one fiftieth of a second. Thus a slowed heart rate, *e. g.*, forty-eight fiftieths of a second, would be positive and so plotted above the X axis, and any value below that of the standard line would be negative and would be plotted below. So as the heart rate increases we have a decrease in the pulse length and a fall in the curve on the coördinate paper, and *vice versa* for a decrease in the heart rate. Each unit along the X axis marks off one heart beat. The dotted line perpendicular to the standard line indicates when the signal or the stimulus

was given. The breathing record was plotted along with the heart rate curve on the same paper and in a similar manner, excepting that different values were given to the spaces and to the base line (the X axis). The latter was given the value of 0 and each space vertical to it the value of one millimeter. The spaces along the X axis indicate the pulse beat as before. Thus the depth of the inspiration is plotted below the standard line and the height of expiration above it. The dotted curve indicates the heart rate curve and the full line the breathing curve. In the discussion we shall mean by the *pulse wave* the long wave in the heart rate including several breaths and Traube-Hering in character. The significance of the *breathing wave* is obvious.

The following are the results of the records as they were plotted:

Retarded Breathing.

No. 1B. No stimulus was given, the subject simply attended to his breathing, making it slower after the signal. He reported a disagreeable feeling from the lack of breath. The result was that we got a gradual decrease in the heart rate beginning at the signal and lasting throughout, and no change in the breathing or pulse waves.

Nos. 3B, 21D, and 37B. The results in these three were all alike. We got a very decided decrease in heart rate, an increased pulse wave, and an increased breathing wave. See plate No. 21D. The introspections in these experiments were the same as in No. 1B.

No. 5B. In this there was a decreased heart rate, an increased breathing wave, but no change in the pulse wave. Besides being retarded the breathing was increased in amplitude.

No. 7B. This was the only record that was taken with the retarded breathing that did not show a decided decrease in the heart rate; there was practically no change in this one, although it showed an increase in the breathing and pulse waves. The deviation of this record from the rest may be accounted for by the increase in amplitude.

Nos. 17D and 19D. These gave only decreased heart rate with practically no change in the breathing or pulse waves.

Increased Breathing.

Nos. 9B and 22D. There were no stimuli in these experiments. The subjects simply increased the rate of their breathing, and in No. 22D the amplitude was increased. The subjects both reported an agreeable feeling seeming to make up for the lack of breath in the previous experiments. In No. 9B the subject felt like laughing during the whole experiment, and in 22D the subject felt like taking a deep breath each time. The result was that we got a marked increase in the heart rate, a decrease in the breathing wave, and a total elimination of the pulse wave. See plate No. 22D.

Sensory Stimuli (Auditory).

No. 2B. The stimulus used in this experiment was listening to a very faint sound. A watch was placed so that the ticks were just audible. The breath was held so that each one was from three to four times as long as a normal breath, and the amplitude was slightly increased. The subject reported a feeling of lack of breath and some strain. The sound was very weak and came and went. The results were a slight decrease in the heart rate and an increase in the breathing and pulse waves. These changes began immediately following the signal. See plate No. 2B.

No. 3B. This was a reproduction of No. 2B. The subject was asked to reproduce the breathing as experienced in No. 2B as nearly as he could from memory. His breath was retarded and gave a fairly good reproduction for this method. It shows in No. 2B how the attention holds down the heart rate curve while in No. 3B without so much attention and with practically the same breathing the heart rate curve rises. See plates 2B and 3B. The subject reported a similar feeling in the two experiments.

No. 6B. The stimulus in this experiment was auditory, the same as in No. 2B. The breathing was very much retarded, especially at the first where it was held for several seconds and the amplitude was somewhat increased. The subject reported a lack of air, a disagreeable feeling from strain about the neck and head, and a tendency to move toward the sound. It resulted

that we had a very decided decrease in the heart rate from the very beginning, gradually becoming less as the subject began to breathe more, but at no time coming back to normal. Both the pulse and breathing waves were increased. The subject moved and took a deep breath during the normal.

No. 7B. In this experiment the subject tried to reproduce the breathing and the physiological conditions as experienced in No. 6B, but it was not a very good reproduction. The breath was simply retarded throughout. The results were that we got an increase in the pulse and breathing waves, but practically no change in the heart rate. The introspections were the same as were given in No. 6B.

No. 12C. The stimulus in this experiment was the same as in No. 2B. Immediately following the signal was a slowing of the breathing with the amplitude remaining about the same. The subject reported that he held his breath with expiration rather than with inspiration. He would lose the sound and then hold his breath until he caught it again. It was disagreeable, but not from the lack of breath. The results were a slight gradual increase in the heart rate from the beginning lasting throughout the first half of the record. In the last half of the record we got a partial return to normal. There was a gradual increase in the breathing and pulse waves throughout.

No. 18D. The stimulus in this experiment was the same as used in No. 2B. Immediately following the signal for about one third of the record, was a slight increase in the breathing rate and a decrease in this amplitude. This was accompanied by an increase in the pulse rate at first followed by a normal rate. The breathing then came back to normal and we had a very rapid increase in the heart rate, not much of any change in the breathing wave, but a total elimination of the pulse wave. After the subject had become quiet and attentive the sound was too loud, so instead of attending merely to the sound, he attended to the change of intensity of the sound. The subject felt a slight strain and lack of breath at first in expectation. He was also conscious of his chest breathing.

No. 19D. This was a reproduction of No. 18D though not a very good one. The breathing was retarded too much and the

amplitude decreased. The results were a decreased heart rate, a decreased pulse wave, but no change in the breathing wave. The subject felt a strain and expectation and lack of breath before starting.

Nos. 32B and 33B. The stimuli used in these two experiments were also the same as used in No. 2B. No. 33B is a continuation of No. 32B with a period of about two minutes between. The work of attending to the watch was kept up during this interval. This time was taken between the records to give the operator time to change the papers on the kymograph. The rate of the pulse did not fall as much as might be expected between the two records, but this can be accounted for by the fact that when the kymograph was not running it was not so difficult to hear the sound. Immediately following the signal we got a decided retardation of the breathing with a decreased amplitude. Corresponding to this we had a decrease in the heart rate for about four breaths; then we got a gradual increase throughout the two records with the exception of the last two breaths which were decidedly retarded and decreased in amplitude, and here we got again a slight decrease in the heart rate. There was a very small breathing wave throughout these two experiments which suffered little change. There was practically no pulse wave. In these two records it will be noticed that the heart rate was at first slowed by the decreased breathing, but as the attention became intense the heart rate was increased in spite of the tendency of the decreased breathing to make it go slower.

Nos. 34B and 35B. These were reproductions of Nos. 32B and 33B. They were very good reproductions with the exception that at the end of 34B the breathing was increased a little when it should have been retarded all the way through. The result was that we got a very much decreased heart rate from the beginning, with a gradual increase corresponding to the increased breathing rate. Beginning with No 35B we got a gradual decrease in the heart rate throughout with increased breathing and pulse waves.

Sensory Stimuli (Visual).

No. 4B. The stimulus used in this experiment was reading at a distance. The subject read a German article at a distance

of about six feet, which made it quite a difficult task. Directly following the signal there was very little change if any in the breathing. This was accompanied by an increase in the heart rate, an increase in the breathing wave, and a decrease in the pulse wave. Then in the last half of the record there came a slight decrease in the amplitude of the breath. Accompanying this we got a decrease in the heart rate until it came back to normal, and stayed there, or a little above, throughout the rest of the experiment. There was a slight increase in the pulse wave. The subject reported some lack of breath, a tendency to move toward the stimulus, eye strain, and some muscular strain of the neck and body. The experiment was more or less disagreeable.

No. 16D. The stimulus used in this experiment was like that used in No. 4B, only made a little more difficult. The subject reported that after about the first quarter of the experiment there was a feeling of lack of breath which steadily grew to the end. Two or three times when he started to breathe he lost his place. He was aware of a slight strain. The breathing rate was increased but a very little but the amplitude was quite perceptibly decreased; this remained uniform throughout the rest of the experiment. The heart rate was increased with surprising rapidity from the giving of the signal. It increased from forty-five fiftieths of a second to thirty-five fiftieths. Toward the end of the experiment the heart rate decreased for just three breaths, rising even to normal rate but quickly dropping back again to its rapid speed. We cannot account for this decreased heart rate for so short a time. There must have been some other change either physiological or mental that the subject did not give in his introspection. This was the only case of the kind. The breathing wave was decreased and the pulse wave was almost eliminated.

No. 17D. This was a reproduction of No. 16D, but the pneumograph was cut off in the first part of the experiment so that we did not get a normal curve. It would be hardly fair to judge this reproduction without the normal portion. As far as the last part was concerned it was well reproduced. The heart rate gradually decreased from the signal to the end. There

was no change in the breathing wave but a decrease in the pulse wave. There was strain and expectation before starting.

Nos. 24B and 25B. The stimuli used in these two experiments were counting time marks that were very close together. The breathing was very nearly normal throughout record No. 24B with the exception of three breaths immediately following the signal, which were retarded. For about one third of 25B, which was a continuation of 24B the breathing was slightly increased in rate and decreased in amplitude. Accompanying this there was a decided increase in the heart rate, no change in the breathing wave, and an elimination of the pulse wave, excepting that which accompanied the three breaths following the signal. Here the heart rate was decreased, and the breathing and pulse waves were increased. In the last two thirds of record No. 25B the breathing was very much retarded, accompanied by a decrease in the heart rate to the normal, which remained so throughout. The breathing and pulse waves were slightly increased compared with the first part. See plate No. 24B.

Nos. 26B and 27B. These were reproductions of 24B and and 25B; they were very good. Following the signal to the end we got an almost identical reproduction with the exception of about four breaths which are indicated by Xs on the plate. In the reproduction of the normal the heart rate was greatly increased thus showing the effect of attention, or a sensory stimulus, upon the heart rate. See plates Nos. 24B and 26B.

Between records 24B and 25B while the operator was changing the paper on the main kymograph there was a period of two minutes, but the work was continuous and kymograph No. 2 was kept running. In the reproduction the same time was taken in changing the papers, and the subjects kept reproducing what was done during the similar interval in the first record. So the reproduction was an accurate one all the way through.

Nos. 28B and 29B. The stimuli in these two experiments were the same as used in Nos. 24B and 25B. The breathing rate after the signal was very irregular, but in all cases except those of two or three breaths, was retarded. In five instances the breath was held for a time equal to three normal breaths.

The result was that we got an increased heart rate which remained throughout the experiment with the exception of the times when the breath was held; at these intervals the heart rate curve went up to normal but soon fell again. There was very little change in the breathing and pulse waves.

Nos. 31B and 32B. These were the reproductions of Nos. 28B and 29B. The results were the same as those obtained in Nos. 26B and 27B only not so marked.

Mental Stimuli (Problems).

No. 8B. In this experiment the subject was given a mental problem. He was told to conjugate a French verb. The breath was both retarded and increased in amplitude. Accompanying this, the pulse wave was decreased, the breathing wave suffered no change, and there was scarcely any change in the heart rate; it stayed practically normal. The subject reported a slight feeling of strain, and some expectation of the signal before starting, but did not notice any change in his breathing.

No. 15C. The problem in this experiment was to recall in chronological order the events of the Civil War. There was no change in the respiration but we got a very rapid increase in the heart rate from the beginning lasting to the end. There was a gradual decrease in the pulse wave until it was entirely eliminated. The breathing wave was not changed. See plate No. 15C. The subject reported that the effort was uniform throughout, there was some strain, and it was somewhat disagreeable in that he could not think of a couple of events. He did not notice any change in the respiration.

No. 20D. The problem in this experiment was to say the alphabet backwards. The breathing rate was slightly increased and the amplitude of the breath was slightly decreased. Accompanying this was a very marked increase in the heart rate, a rapid decrease in the pulse wave, and but little change in the breathing wave. The subject reported that he visualized the alphabet from a sheet of paper on which he had been working with the alphabet on it, but it was difficult to do this all the way through. There was some feeling of strain and lack of breath before starting.

No. 23B. The problem in this experiment was to begin with

nineteen and add rapidly by seventeens. The breathing was nearly normal, only retarded for a couple of times and those only for a couple of breaths. There was a gradual increase in the heart rate throughout with very little change in the breathing wave. The pulse wave was very slightly increased. The subject took a long breath just before starting. It was difficult to begin the problem but grew easier toward the end. Just at the end he forgot his last number, and then had to work extra hard to recall it again. See plate No. 23B.

No. 36B. The problem in this experiment was to begin with seventeen and add by nineteens. The breathing rate was normal and the amplitude of the breath was increased. Directly following the signal we got a quick increase in the heart rate followed by a slight return which stayed uniform throughout the rest of the experiment but at no time came back to normal. The breathing and pulse waves were little affected. The subject was startled at the signal to begin.

Summary.

We may summarize the results as follows: With close visual attention the breathing is uniformly decreased in amplitude. In rate it is sometimes increased, sometimes decreased, and sometimes not changed at all. With auditory attention it is nearly always decreased in rate, but changed irregularly in amplitude. The breathing in the kind of central attention that we used is very little changed. These changes are probably adaptive; they remove a source of disturbance. Deep breathing, with its accompanying movements, would interfere with looking; rapid breathing interferes more with the listening.

With the effort of attention the strain tends to increase the heart rate. Increased breathing, either in rate or amplitude, tends to increase the heart rate. Restricted breathing, either in rate or amplitude, tends to decrease the heart rate. For the latter reason one often finds a decreased heart rate with sensory attention, particularly at the first. With central attention the heart rate is regularly increased.[1]

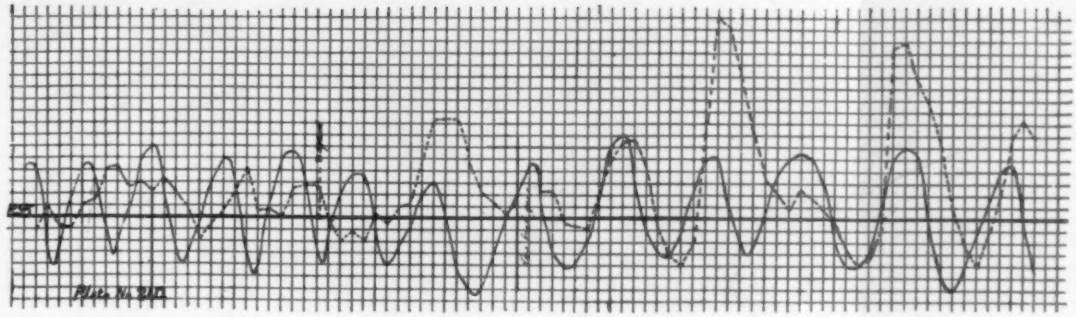
[1] This physiological effect of breathing may account for the divergent reports in the literature. So some writers might find a slowed pulse at times with

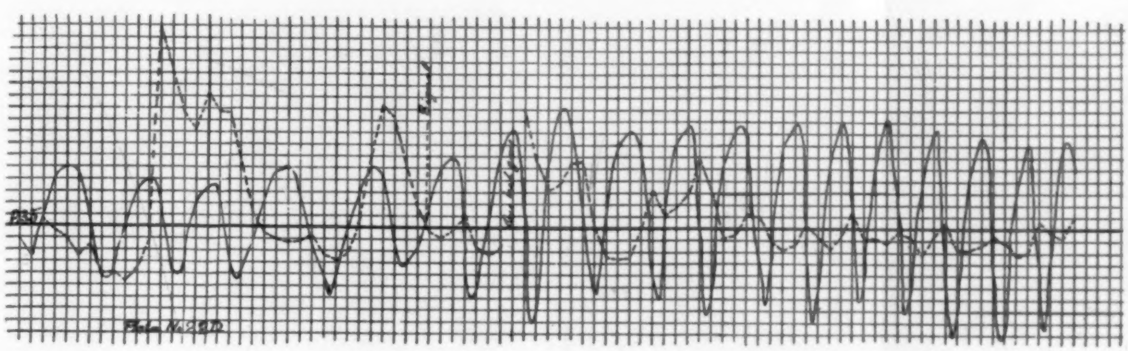
With restricted breathing and decreased heart rate the pulse wave is markedly increased; with increased breathing and increased heart rate we get a decreased pulse wave. The heart rate change seems to be the more prominent factor with the pulse wave. It tends to decrease the wave when it itself is increased in attention in spite of the retarded breathing, but the lack of breath probably has some influence.

In studying the expression of the emotions it would be desirable to know more thoroughly the relation of the breathing and circulation changes in sorrow, joy, etc. An attempt will be made to do this later.

strain. Stevens concludes that the psychophysical processes of sensation are different with visual, auditory, and tactual stimuli. His comparatively rough method of taking results makes an exact investigation impossible; but a study of his published records suggests that, as far as any conclusion can be drawn from them, the differences are due to these changes in breathing.

Plate I.





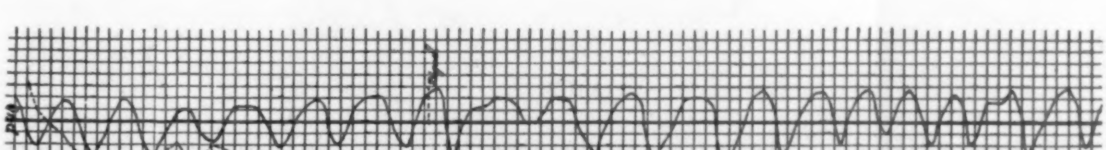
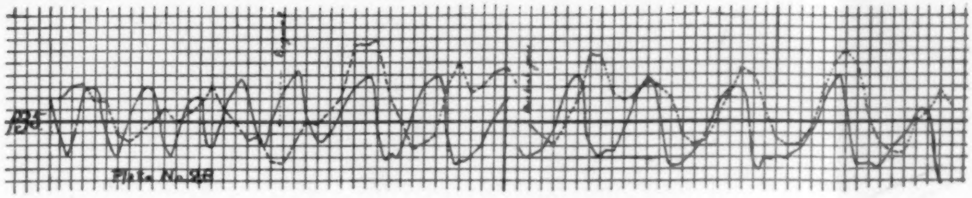
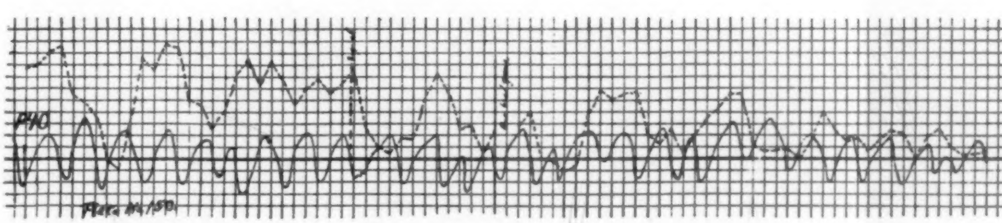


PLATE II.





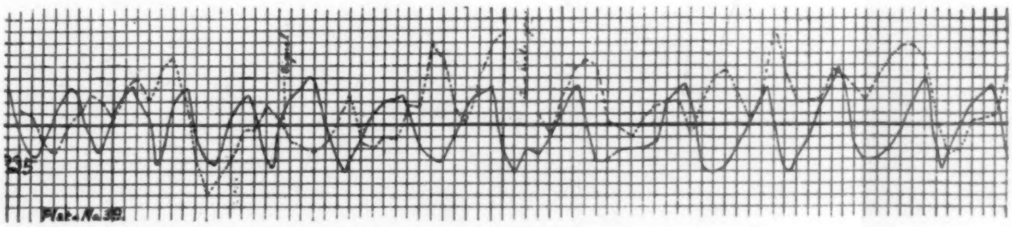


PLATE III.